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SKETCH

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BY THOMAS THOMSON, M. D., F. R. S., L. & E., &c. &c. &c.

REGIUS PROFESSOR OF CHEMISTRY IN THE UNIVERSITY OF GLASGOW.

THE cradle of the human race was beyond dispute the southern portion of Asia—a delightful climate, where the original inhabitants of the earth first lived and multiplied. Chaldea and India had attained a high degree of civilization long before the Greeks and Romans had begun to emerge from a state of barbarism; but we know comparatively little of the attainments in science which these nations had reached. The few facts which have been gleaned since the East India Company has established its dominion over Hindostan, will be stated in the course of this sketch. We are equally ignorant of the progress which mathematical and physical inquiries had made in China—not one of the treatises on Mathematics, Arithmetic, and Astronomy in the Chinese language having been translated into any of the languages of modern Europe. But the resemblance between the Chinese and the ancient Egyptians, is so very striking, and so complete, that it is difficult to avoid suspecting that they had a common origin. If this were so, China, from its contiguity to India and Chaldea, and from the delicious nature of its climate, must have been first furnished with inhabitants. And the Egyptians, if ever they were a colony of Chinese, must have been transplanted into Egypt long before the commencement of history. It was from Egypt that the Greeks drew the first rudiments of their mathematical and physical science; and the scientific acquisitions of that singular people constitute every thing that we know respecting the progress which the ancients had made in the investigation of nature.

From the genial climate of the early inhabitants of the east, and the nature of the life which they led, it was natural to expect that the magnificent spectacle of the heavens would speedily attract their attention. We are certain that the Chaldeans made astronomical observations at least as early as the 27th and 28th years of the era of Nabonasser; that is to say, 719 and 720 years before the commencement of the Christian era: for Ptolemy makes use of three observations of the eclipses of the moon, which took place during these years, and which he found in their records. Diogenes Laertius informs us that the Egyptians had preserved in their annals an account of 373 eclipses of the sun, and 832 of the moon, which had happened before the arrival of Alexander the Great in their country. Now these eclipses required between 1200 and 1300 years to happen. Alexander's visit to Egypt took place in the year 331 before the Christian era. If we add this number to the length of time during which the Egyptians continued to observe the eclipses of the sun and moon, we obtain 1631 years before the commencement of the Christian era for the period at which the Egyptians began to record their observations. This period is rather more than a century after the death of Moses, and is about twenty-four years before the institution of the Olympic games; constituting but a small part of the 48,863 years during which they boasted that they had been engaged in making astronomical observations. But this was obviously a fable, invented for the purpose of raising themselves in the opinion of the Macedonian conqueror.

What progress the Chaldeans and Egyptians had made in astronomy, it is hard to say. They certainly had become acquainted with the planets; but whether the Egyptians had discovered, as Macrobius assures us, that Mercury and Venus revolve round the sun, is not so clear. Their notions respecting the length of the solar year, and the mean length of the lunation, must have been a near approximation to the truth. This is evident from the famous Chaldean period called *Saros*. It consisted of 223 lunar months, at the end of which the sun and moon were in the same situation with respect to each other, as when the period began. This period includes a certain number of eclipses of each luminary, which are repeated every saros in the same order.

The Chaldeans appear to have divided the day into twelve hours, and to have constructed sun-dials for pointing out the hour. The sun-dial of Ahaz is mentioned in the Old Testament, on the occasion of the recovery of Hezekiah; but nothing is said about its construction. Undoubtedly, however, such sun-dials would require a certain knowledge of gnomonics, which therefore the Chaldeans must have possessed.

That the Egyptians had made some progress

in mathematics admits of no doubt, as the Greeks inform us that they derived their first knowledge of that branch of science from the Egyptian priests. But that the mathematical knowledge of the people could not have been very extensive, is evident from the ecstasy into which Pythagoras was thrown, when he discovered that the square of the hypothenuse of a right angled triangle is equal to the square of the two sides. For ignorance of this very elementary, but important proposition, necessarily implies very little knowledge even of the most elementary parts of mathematics.

It was in Greece that pure mathematics first made decided progress. The works of three Greek mathematicians still remains, from which we have obtained information of all or almost all the mathematical knowledge attained by the Greeks.—These are Euclid, Appolonius, and Archimedes.

Euclid lived in Alexandria during the reign of the first Ptolemy. Nothing whatever is known respecting the place of his nativity; though it is certain he lived in Greece, and that he died in Egypt, after the foundation of the celebrated Alexandrian school. He collected all the elementary facts known in mathematics before his time, and arranged them in such an admirable order—beginning with a few simple axioms, and deducting from them his demonstrations, every subsequent demonstration depending on, and rigidly deduced from those that immediately precede it—that no subsequent writer has been able to produce any thing superior or even equal. His Elements still continue to be taught in our schools, and could not be dispensed with, unless we were to give up somewhat of that rigor which has been always so much admired in the Greek geometricians. Perhaps, however, we carry this admiration a little too far. The geometrical axioms might be somewhat enlarged, without drawing too much upon the faith of beginners. And were the method followed, considerable progress might be made in mathematics without encountering some of those difficult demonstrations that are apt to damp the ardor of beginners.

The elements of Euclid consist of thirteen books. In the first four he treats of the properties of lines, parallel lines, angles, triangles, and circles. The fifth and sixth treat of proportions and ratios.—The seventh, eighth, ninth, and tenth treat of numbers. The eleventh and twelfth treat of solids; and the thirteenth of solids; also of certain preliminary propositions about cutting lines in extreme and mean ratio. It is the first four books of Euclid chiefly that are studied by modern Geometricians. The rest have been, in a great measure, superseded by more modern improvements.

Appolonius was born at Perga in Pamphylia, about the middle of the second century before the Christian era. Like Euclid, he repaired to Alexandria, and acquired his mathematical knowledge from the successors of that geometrician. The writings of Appolonius were numerous and profound; but it is upon his Treatise on the *Conic Sections*, in eight books, that his celebrity as a mathematician chiefly depends.

The Conic Sections, which, after the circle, are the most important of all curves, were discovered by the mathematicians of the Platonic school; though who the discoverer was is not known. A considerable number of the properties of these curves were gradually developed by the Greek geometricians. And the first four books of Apollonius are a collection of every thing known respecting these curves before his time. The last four books contain his own discoveries. In the fifth book he treats of the greatest and smallest lines which can be drawn from each point of their circumference, and many other intricate questions, which required the greatest sagacity and the most unremitting attention to investigate. The sixth book is not very important nor difficult; but the seventh contains many very important problems, and points out the singular analogy that exists between the properties of the various conic sections. The eighth book has not come down to us. The fifth, sixth, and seventh books were discovered by Borelli, in Arabic, in the library of the grand duke of Tuscany. He got them translated, and published his translation, with notes and illustrations, in the year 1661. Dr. Halley published an edition of Appolonius in 1710, and has supplied the eighth book from the account given by Pappus of the nature of its contents.

Archimedes was, beyond dispute, the greatest mathematician that antiquity produced. He was born in Sicily about the year 287 before the Christian era, and is said to have been a relation of Hiero, King of Syracuse. So ardent a cultivator was he of the mathematics, that he was accustomed to spend whole days in the deepest investigations, and was wont to neglect his food, and forget his ordinary meals, till his attention was called to them by the care of his domestics. His studies were particularly directed to the measurement of curvilinear spaces; and he invented a most ingenious method of performing such measurement, well known by the name of the *Method of Exhaustions.*

When it is required to measure the space bounded by curve lines, the length of a curve, or the solid bounded by curve surfaces, the investigation does not fall within the range of elementary geometry. Rectilinear figures are compared on the principle as superposition; but this principle cannot be applied to curvilinear figures. It occurred to Archimedes, that, by inscribing a rectilinear figure within, and another without the figures, two limits would be obtained, the one greater and the other smaller than the area required. It was evident that, by increasing the number, and diminishing the sides of these figures, these two limits were made continually to approach each other. Thus they came nearer and nearer to the curve area which was intermediate between them. He observed, by thus increasing the number of sides for a great number of times successively, that he approached a certain assignable rectilinear area, and could come nearer to it than any difference how small soever. It was evident that this rectilinear area was the real size of the curvilinear area to be measured. It was in this way that he found that two-thirds the rectangle under the abscissa and ordinate of a parabola, is equal to the area contained by the abscissa and ordinate, and that part of the circumference of the parabola lying between them. In the same way he obtained an approximate measure of the area of the circle, demonstrating that if the radius be unity, the circumference is less than $3\frac{10}{70}$, and greater than $3\frac{10}{71}$. His two books on the sphere and

cylinder were conducted by a similar method of reasoning. He measures the surface and solidity of these bodies, and terminates his treatise by demonstrating that the sphere (both in surface and solidity) is two-thirds of the circumscribed cylinder.

In the same spirit his Treatise on Conoides and Spheroids was conducted. These names he gave to solids formed by the revolutions of the conic sections round their axis. We pass over his researches on the spiral of Archimedes, as it is usually called, though in reality discovered by Conon, one of his friends; but must notice the Treatise entitled *Psammites*, or *Arenarius*. Some persons had affirmed that no number, however great, was sufficient to express the number of grains of sand situated on the sea-shore. This induced Archimedes to write his treatise, in which he demonstrated that the fiftieth term of a ducuple increasing progression is more than sufficient to express all the grains of sand contained in a sphere, having for its diameter the distance between the earth and the sun, and totally filled with grains of sand. The treatise is short; but abstruse in consequence of its imperfect method of expressing numbers employed by the Greeks. Were our figures substituted for the Greek letters, the reasoning would be sufficiently simple and clear.

Archimedes did not confine himself to pure mathematics; he turned his attention likewise to mechanics, and may, in some measure, be considered as the founder of that important branch of physical science. He first laid down the true principles of *statics* and *hydrostatics*. The former he treats in his work entitled *Isorropica* or *De Equiponderantibus*. His statics are founded on the ingenious idea of the *centre of gravity*, which he first conceived, and which has been so advantageously employed by modern writers on statics. By means of this principle, and a few simple axioms, he demonstrates the reciprocity of the weight, and the distance in the lever and in balances, with unequal arms. He determined the centre of gravity of various figures, particularly of the parabola, with great ingenuity.

His discoveries in hydrostatics were the consequence of a query put to him by King Hiero.—This monarch had given a certain quantity of gold to a jeweler, to fabricate a crown, and he suspected that the artist had purloined a portion of the gold, and substituted silver in its place.—Archimedes was requested to point out a method of determining how much gold had been purloined, and how much silver substituted. The method, it is said, occurred to him all at once, while in the bath, and he was so transported with joy, that he ran naked through the streets of Syracuse, crying out, εὕρηκα, εὕρηκα, *I have found it, I have found it*. The discovery with which he was so deservedly delighted was this, "Every body plunged into a fluid loses as much of its weight as is equal to the weight of a quantity of the fluid equal in bulk to the body plunged in." This discovery furnished him with the method of determining the specific gravity of pure gold and pure silver. These being known, he had only to take the specific gravity of the crown, which (supposing no alteration in volume when the two metals are melted together) would enable him to discover how much gold and how much silver it contained.

This first principle being known, Archimedes deducted from it various other well-known hydrostatical principles, which he consigned in the first book of his treatise *de Incidentibus in Fluido*.—The second book of that treatise is occupied with various difficult questions respecting the situation and stability of certain bodies immersed in a fluid.

The ancients ascribe to him the intention of forty remarkable mechanical contrivances; but nothing more than some obscure notices of two or three of them have come down to us. His sphere, a machine by which he represented the movements of the stars and planets, is one of the most celebrated. It has been noticed by grave philosophers, and sung by poets, as may be seen in the following epigram of Claudian:

> Jupiter, in parvo cum cerneret æthera vitro,
> Risit, et ad superos talia verba dedit:
> Huccine mortalis progressa potentia curæ;
> Ecce Syracusii ludimur arte senis.

Archimedes wrote a description of this machine, under the name of Sphæropœia; but it is lost, and with it every thing respecting the nature of the sphere has perished.

The burning mirrors, by which he is said to have set fire to the Roman vessels in the harbor of Syracuse, were long considered as fabulous.—But Buffon showed how, by placing a number of small mirrors so that every one of them should reflect the image of the sun to the same point, heat enough might be produced to kindle wood at the distance of 140 feet.

The protracted defence of Syracuse against the Romans, chiefly in consequence of the wonderful mechanical inventions of Archimedes, is too well known to be enlarged on here.

If we except the discoveries of Archimedes in statics and hydrostatics, hardly any other branch of physical science was much cultivated by the ancients. They have made, indeed, considerable progress in the knowledge of acoustics, so far as music is concerned. In optics they can scarcely be said to have made any progress of consequence; and, in astronomy, very little till the time of Hipparchus, who may be considered as, in some measure, the founder of that sublime science.

After these preliminary remarks on the progress of physical science among the ancients, we shall proceed to take a brief view of the advances which have been made in it since the revival of letters, and to describe the present state of the various branches into which it has been divided, as far at least as is consistent with the very limited length to which our observations can be permitted to extend.

There are only two methods by which the physical sciences can be advanced. They are, 1. Observation and experiment; 2. The application of mathematical reasoning to deduce new truths from facts already established. We shall take a brief view of these two instruments of investigation in the first place; beginning with mathematics; because they are first employed.

OF MATHEMATICS.

The object of mathematics is the measurement or comparison of *quantity*. Now, there are two

kinds of quantity, namely, *number* and *surface*.—The branch of mathematics which treats of numbers is called *arithmetic*, that which treats of *surfaces*, or rather of *space*, is called *geometry*.—There is a third branch of mathematics, which treats of quantity in a general way, and which, therefore, applies equally to arithmetic and geometry. To this branch, the name of *algebra* has been given.

ARITHMETIC.

The ancients employed the letters of the alphabet to represent numbers. This method seems to have originated with the Egyptians or Phenicians, or, at any rate, the Greek mode of expressing numbers was obviously borrowed from the Hebrew.

The decimal mode of numeration has been adopted by almost all nations, evidently because man has ten fingers, and because men were in the habit of reckoning on the fingers, and after coming to an end, they begin again. If the number of the fingers had been twelve instead of ten, the mode of numeration would certainly have been duodecimal instead of decimal—and this mode would have had its conveniences, which the decimal mode wants.

The Hebrew alphabet has twenty-two letters. The first nine of these letters denoted the nine digits; thus א, 1; ב, 2; ג, 3; ד, 4; ה, 5; ו, 6; ז, 7; ח, 8; ט, 9; the next nine letters denote the nine tens; thus, י, 10; כ, 20; ל, 30; מ, 40; נ, 50; ס, 60; ע, 70; פ, 80; צ, 90.—The rest of the alphabet consisted only of four letters; but there are five of the letters that have a different form when at the end of a word. These are, כ, מ, נ, פ, צ, which are then written ך, ם, ן, ף, ץ. By means of the last four letters, and these five final letters, they express the nine hundreds: thus, ק, 100; ר, 200; ש, 300; ת, 400; ך, 500; ם, 600; ן, 700; ף, 800; ץ, 900.

The Hebrews wrote from right to left, contrary to our method. Hence, when two numbers are placed together, that on the right hand stands for tens, and that on the left for units. Thus, יב is 12; קח is 105, and so on.

The Arabians likewise represent numbers by the letters of the alphabet, precisely as the Hebrews: the first nine letters representing the nine digits; the second nine, the nine tens; and the third nine, the nine hundreds. The twenty-eighth letter of the Arabian alphabet represents 1000.

The Greeks obviously borrowed their mode of expressing numbers from the Hebrews or Phenicians. The first five letters of the Greek alphabet represent the first five digits. As the Greeks have no letter corresponding to the Hebrew ו, which denotes six, they introduced for that purpose the mark ϝ or ς, which they called episemum. The next three letters, ζ, η, θ, represent 7, 8, 9, corresponding with the Hebrew characters ז, ח, ט, which stand for the same numbers. The next eight letters of the alphabet correspond with those of the Hebrew, and represent the numbers, 10, 20, 30, 40, 50, 60, 70, 80. There being no letter in the Greek alphabet corresponding with the Hebrew צ, which stands for 90, the Greeks introduced for that purpose the character ϟ, or Ϙ, or ϙ, to which they gave the name of *hoppa*, probably from the Hebrew letter koph. The last eight letters of the Greek alphabet, beginning with ρ, denotes 100, 200, 300, 400, 500, 600, 700, 800. To stand for 900, they invented the mark ϡ, which they called sanpi. Thousands were denoted by the letters of the alphabet with an accent under them. Thus, α was 1; α, 1000; γ, 3, and γ, 3000, and so on.

The most defective mode of notation is that of the Romans, who employed the following letters: I for 1; V for 5; X for 10; L for 50; D for 500; and M for 1000. By means of these symbols, they contrived to represent moderate numbers. But such an imperfect method was incompatible with almost any progress in the most common rules of arithmetic. Accordingly, the Romans produced no mathematicians, nor any person skilled in the science of numbers.

The mode of expressing all numbers by the ten symbols, 1, 2, 3, 4, 5, 6, 7, 8, 9, 0, with which every body is familiar, seems to have originated in India. In that country it has been used from time immemorial. Not the smallest proof remains that they ever made use of the letters of the alphabet for that purpose. The Arabians call the decimal scale of arithmetic, *Hindosi*, or Indian arithmetic, clearly pointing out the source from which their mode of notation was derived. Whether the Indians were the original contrivers of this astonishing improvement, or whether they borrowed it from some other nation, we have no means of determining. The Chinese, it is said, possess treatises on arithmetic and geometry; but as no translation, or even abstract, of the contents of any such work has been published in Europe, we are ignorant how far their knowledge of these subjects extends. Had they been acquainted with the Indian mode of notation, it is hardly possible, considering the numerous mercantile transactions which had taken place between them and Europeans, that it should have entirely escaped our knowledge.

The Arabians became acquainted with the Indian notation when they began to prosecute science under the caliphs, and they had liberality enough to be sensible of its superiority, and to adopt it. Along with the Mahomedan religion, it made its way into Spain; which during the dark ages, was the most enlightened country in Europe, and to which all those resorted from every country, who wished to be initiated in the rudiments of science.

About the beginning of the tenth century, the monastery of Fleuri, of the order of St. Benedict, had for its abbot, Abbon, who was a zealous cultivator of the sciences, particularly of those connected with mathematics. He rendered his monastery a celebrated school of knowledge and piety; and all the monks of St. Benedict, who gave proofs of abilities, were sent thither to receive instruction. Among these was Gerbert (afterwards pope Sylvester II.), a native of Auvergne. After having acquired all the knowledge that Fleuri could furnish, he obtained leave to repair to Spain, where two celebrated schools at that time existed at Cordova and Grenada, which belonged to the Mahomedan conquerors of that country. These schools were much frequented both by Mahomedans and Christians. Gerbert made such progress in mathematical knowledge, that he soon surpassed his masters. Arithmetic, music, geometry,

and astronomy, occupied his attention; and had on his return to France, he communicated to his countrymen the knowledge which he had thus acquired. But the greatest boon which he conferred on Europe was the introduction of the Indian numbers, which he found in common use among the scientific Mahomedans in Spain. The date of this introduction must be fixed between the years 970 and 980.

With a knowledge of the Indian figures, the rules of arithmetic became also plain, and came to be familiarly understood. It is obvious from the translation of the Lilawati, by Dr. Taylor, a treatise on Arithmetic and Geometry, by Bhascora Acharya, who was born in the year 1114, that in his time, the four common rules of arithmetic, the rule of three, the management of vulgar fractions, the method of extracting the square and cube roots, and most of the various rules at present met with in our books of arithmetic, were known in India. No doubt they were also taught at Cordova and Grenada, and brought by Gerbert into France. From that date, then, we may reckon the introduction of arithmetic, such as we have it at present, into common use; certainly one of the greatest boons ever conferred upon mankind.

The next great improvement introduced into arithmetic, was by John Muller, better known by his Latinized name of Regiomontanus. He was born in Konigsburg, a small town in Franconia, in the year 1436. Scarcely had he reached his 14th year, when he became enamored with the charms of mathematics and astronomy. He became the favorite pupil and friend of Purbach, who at that time was possessed of reputation. With him he resided till the time of his death engaged chiefly in astronomical observations. The labours of Regiomontanus were incessant, and the reputation which he acquired was deservedly high. But we mention his name here, because it was to him that arithmetic owes the introduction of decimal fractions. Thus he gave to numerical computation its utmost degree of simplicity and enlargement which it seems capable of reaching.

Regiomontanus was incessantly occupied with astronomical observations and calculations, which of course, rendered the perfection of trigonometry an object of the most importance: he did accordingly bring it nearly to the same state of simplicity which it at present possesses. But the calculations necessary for such purposes are exceedingly laborious, and their labour necessarily increased as astronomy advanced, because it became essential to obtain more and more accurate results. The sines and tangents of angles could not be expressed with sufficient correctness, without decimal fractions, extending to five or six places; and when to three such numbers, a fourth proportional was to be found, the work of multiplication and division became exceedingly laborious. About the end of the 16th century, the time and labor necessarily spent on such calculations, had become extremely burdensome to mathematicians and astronomers.

Napier of Merchiston, whose mind seems to have had a bent towards arithmetical pursuits, was the person to whom the happy thought occurred of a method by which that labor might be prodigiously diminished by substituting addition and subtraction, for multiplication and division; this he did by the discovery of logarithms, and the constructions of logarithmic tables by himself, Briggs, Gellibrand, &c.

He observed that when the numbers to be multiplied or divided, were parts of a geometrical series, or progression, provided we know the progression, the product or quotient might be got at once by inspection. Thus the 3d term of a progression, multiplied into the 5th term, would make the 8th term; and the 12th term, divided by the 3d term, would make the 9th term. He satisfied himself that all numbers might be intercalated between the terms of a geometrical progression; and he hit upon a most ingenious way of proving the truth of his proposition. What he called logarithms consist of numbers in the arithmetical progression, corresponding to all numbers supposed to exist in a geometrical progression, and various systems of logarithms may be constructed. The one which first occurred to the inventor, though the simplest, was not so convenient, as the one which occurred soon after to himself and his friend Briggs, and according to which the tables now in use have been constructed. It is plain, that if we add together two logarithms, the logarithm constituting the sum of two, will correspond with the number which would be obtained by multiplying the two numbers together; hence, by adding and substracting logarithms, and looking up for the new logarithm in the tables we find over against it, the number which would be obtained, if the numbers whose logarithms we use, were multiplied or divided by each other. What a prodigious saving of time and trouble is thus accomplished, must be evident from first sight. Nor have the benefits conferred upon mathematics by logarithms been confined to this benefit, great as as it is; they have spread themselves upon other branches of the science. In 1618, Briggs published the logarithms of the first 1000 numbers, under the title of *Logarithmorum chilias prima;* in 1624, he published the logarithms of all numbers, from 1 to 20,000, and from 90,000 to 100,000; all calculated to the 14th decimal place. Death prevented Briggs from finishing his plan, but it was completed by Gellibrand, and published by him in his Trigonometria Brittannica, in 1633.

II. GEOMETRY.

We have seen the progress which geometry had made among the ancients, and how Archimedes by the method of exhaustions, had succeeded in measuring spaces bounded by curve surfaces.—This method though satisfactory, was exceedingly laborious, and being purely synthetical, it did not enable us to apply it to other discoveries of the same kind. A more compendious, and more analytical method was much wished for: and this great step was made by Cavalleri, in the year 1635, in his book entitled, *Geometria indivisibilibus continuorum nova quadam ratione promota.* Cavalleri was born in Milan, in the year 1598.—He entered into the society of Jesuits, was sent to the university of Pisa, where he acquired his geometrical knowledge, and was afterward professor of astronomy in the university of Bologna.

Cavalleri proceeded in his geometry of indivisibles, on the following principles: areas may be considered as made up of an infinite number of

parallel lines; solids, of an infinite number of parallel planes; and lines, of an infinite number of points. Thus, the cubature of a solid was reduced to the summation of a series of planes; and the quadrature of a curve, to the summation of a series of ordinates. Now the rule for summing an infinite series of terms in arithmetical progression, had been long known, and the application of it to find the area of a triangle, according to the method of indivisibles, was a matter of no difficulty. The next step was, supposing a series of lines in arithmetical progression, and squares to be described on each of them, to find what ratio the sum of all these squares bears to the greatest square, taken as often as there are terms in the progression. Cavalleri showed that when the number of terms is infinitely great, the first of these sums is just one third of the second; this evidently led to the cubature of many solids. Proceeding one step further, he sought for the sum of the cubes of the same lines, and found it to be one-fourth of the greatest, taken as often as there are terms; and continuing his investigations, he was able to assign the sum of the *n*th power of a series in arithmetical progression, supposing always, the difference of the terms to be infinitely small, and the number of terms to be infinitely great. The number of curious results obtained from these investigations was prodigious. It may be considered as constituting as great a step over the calculus of exhaustions, as the integral calculus, was over the geometry of indivisibles.

The next general step, (for we are under the necessity of passing over many individual discoveries of great importance,) in the extension of mathematics, was the *arithmetic of infinites* of Dr. Wallis, first published in the year 1655.—Wallis was a native of Kent, where he was born at Ashford, in the year 1616. He was appointed Savillian professor of mathematics at Oxford, in 1649; which place he occupied till the year 1703, when he died.

The origin of the arithmetic of infinites, was an attempt by Wallis to discover the quadrature of the circle, after he became acquainted with the method of indivisibles of Cavalleri. He began by observing, that if we have a series of numbers, arithmetically proportioned (the natural numbers for example) beginning with 0, and proceeding regularly on, the sum of this series, is equal to half the sum of the last term, repeated as many times as there are terms in the series—Thus,

$$\frac{0+1}{1+1}=\frac{1}{2}; \quad \frac{0+1+2}{2+2+2}=\frac{3}{6}=\frac{1}{2}; \quad \frac{0+1+2+3}{3+3+3+3}=\frac{6}{12}=\frac{1}{2}.$$

From this it follows, that a triangle is half a parallelogram, on the same, or equal basis, and between the same parallels. For a triangle may be considered as composed of an infinite number of lines, beginning at a point or 0, and increasing arithmetically, the greatest of which is the base; while a parallelogram consists of an infinite number of lines of equal length, and all equal to the base. From this analogy he deduced the quadrature of a variety of figures.

He next shows that the sum of an infinite series of numbers, beginning with 0, and proceeding as the squares of the natural numbers, that is, the series, 0, 1, 4, 9, 16, 25, 36, 49, &c, is to the last term, repeated as often as there are terms in the series, as 1 to 3. From this he shows that the compliment of a semi-parabola is to the par. allelogram contained between the corresponding abscis and ordinate, as 1 to 3; and deduces from the same analogy, many other of the quadratures of curvilinear spaces.

He then shows that the sum of an infinite series of quantities, increasing from 0, as the cubes of the natural numbers, or the series 0, 1, 8, 27, 64, 125, 216, &c, is the sum of the last term, repeated as often as there are terms in the series, as 1 to 4. This analogy furnishes the quadrture of many figures.

But it would be too tedious to attempt an analysis of the whole of this curious book, which may be considered as furnishing the first germ of the integral calculus. There are however a few other particulars which it would be improper to omit. He was led by analogy to express the denominators of fractions by means of negative powers.—The numbers

$$x^3, x^2\ x^1, x^0, (\text{or } 1,) \frac{1}{x}, \frac{1}{x^2}, \frac{1}{x^3} \text{ \&c.}$$

are in continued geometrical progression. This led him to express them in this way, x^3, x^2, x^1, x^0, x^{-1}, x^{-2}, x^{-3}. Simple as this improvement may appear, it has led to consequences of the greatest importance. It put him in possession of the measure of every space, the elements of which are reciprocally as any power of the abscissa. A prodigious number of new discoveries was the necessary consequence of these new views.

Most of the discoveries made by other mathematicians immediately after the publication of the Arithmetic of Infinites, were little else than the developments of the views of Wallis. Neil showed that the cubic parabola might be rectified by one of Wallis's methods; and Van Heuract applied the same method to the rectification of several other parabolas.

We must pass over the discoveries of Barrow, though they are highly entitled to attention, and hasten to Sir Isaac Newton, who was destined to change the face of physical science—and who hitherto stands alone among mankind as the inventor of a theory to explain the motions of the heavenly bodies, which has stood the test of the most rigid examination, and which every subsequent improvement and discovery has served only to confirm and establish.

Newton was born in the year 1642, on Christmas, old style, at Woolsthorpe, in the county of Lincoln. The family came originally from Newton, in Lancashire, and it appears from a letter published by Dr. Reid, that, by the mother's side, he was of Scottish extraction. When young, he discovered a surprising turn for making models of mechanical instruments. His mathematical knowledge came to him with incredible facility. Euclid he merely turned over, considering it to contain nothing but common things. The first mathematical book that he read was Descartes' Geometry, and the second Wallis's Arithmetic of Infinites. On these books he wrote commentaries as he read them, and reaped a rich harvest of discoveries; or, more properly speaking, he made almost all his mathematical discoveries as he proceeded in their perusal. In 1667, he was elected fellow of Trinity College, Cambridge, and in

1669, Dr. Brown resigned his mathematical professorship to him. He resigned this chair in 1703, two years after Mr. Whiston had taught for him. He died in the year 1727. He was a member of the convention parliament, in 1789, and was, for a good many years, master of the mint.

His first mathematical discovery of importance was the binomial theorem. But his great mathematical discovery was *method of fluctions*, or the *integral and differential calculus*, as it is called on the continent. This, he assures us, he was in possession of as early as 1665 or 1666. Barrow informs us, that soon after that period, there was put into his hands, by Newton, a manuscript treatise, the same which was afterward published under the title of *Analysis per Æquationes Numero terminorum infinitas*, in which the principle of fluxions, though not fully explained, is yet distinctly pointed out. Barrow strongly exhorted Newton to publish this treasure to the world; but an excess of modesty, almost amounting to a disease, prevented his compliance.

For a long time, the mathematical discoveries of Newton were known only to his friends. The first work in which he communicated any thing to the world on the subject, was the first edition of the *Principia*, in 1687, in the second lemma of the second book. The principle of the calculus is there pointed out; but nothing is said of the algorithm, which is so essential to that calculus. This only became known to the world in 1693, by the publication of the second volume of Wallis's works (p. 390, &c.) There is no evidence that this notation existed earlier than 1692, though it is highly probable that it did. It was no less than ten years after this, or in 1704, that Newton himself published a work on the new calculus, his *Quadrature of Curves*, more than twenty-eight years after it had been written.

These discoveries, however, even before they were committed to the press, could not remain altogether unknown in a country where mathematics were cultivated with such zeal and success.—Barry communicated them to Oldenburgh, secretary of the Royal Society. By him they were partly communicated to Mr. James Gregory, and likewise to Leibnitz, who had become acquainted with Oldenburgh during a visit which he had made to England in 1673. At that time Leibnitz knew very little of the mathematics; but having afterward turned his attention to that science, he was soon in a condition to make discoveries. One of the first of these was a very remarkable series, which gives the value of a circular arch in terms of the tangent. This series he communicated to Oldenburgh, in 1674, and received, in return, an account of the progress made by Newton and Gregory, in the invention of series. In 1676, Newton described his method of quadratures, at the request of Oldenburgh, in order that it might be transmitted to Leibnitz.

The method of fluctions is not communicated in these letters; nor are the principles in any way suggested; though there are, in the last letter, two sentences, in transposed characters, which ascertain that Newton was then in possession of that method, and employed, in speaking of it, the same language in which it was afterward made known. In the following year, Leibnitz, in a letter to Oldenburgh, introduced differentials, and the method of his calculus, for the first time.—This letter clearly proves, that in 1677 Leibnitz was in full possession of the principles of his calculus, and had even invented the algorithm and notation.

From these facts, it cannot, we think, be doubted, that Newton was the first inventor of the fluxionary calculus; but that nothing was communicated to Leibnitz regarding the principles of that analysis. Leibnitz, therefore, when he invented the differential calculus, was not assisted by any communications that could give him any idea of what had been done by Newton. Newton was the first discoverer, and Leibnitz the second; both were originals, and both independent of each other. The algorithm of Leibnitz was much more perfect than that of Newton; and it had the great advantage of being first made known to the world—an account of it having been inserted in the first volume of the *Acta Eruditorum*, for 1684.

Thus, while Newton's discovery was known only to a few friends, Leibnitz's was rapidly spread over the continent. John and James Bernoulli joined their talents to those of the original inventor, and illustrated the new method by the solution of a great number of difficult and interesting problems. Leibnitz's methods acquired perfection, while those of Newton remained unknown. The first work on the new geometry was by Craig, who drew his information from the writings of Leibnitz and his friends.

Nothing, however, like hostility appeared between the two great discoverers, and Newton in the passage of the Principia above referred to, gives a highly favorable opinion on the subject of the discoveries of Leibnitz. A remark of Fatio de Duillier, in a paper presented to the Royal Society in 1699, lighted up a flame which a whole century has been scarcely sufficient to extinguish. In a paper on the line of swiftest descent, there occurred this sentence, "I hold Newton to have been the first inventor of this calculus, and the earliest by several years, induced by the evidence of facts; and whether Leibnitz, the second inventor, has borrowed any thing from the other, I leave to the judgement of those who have seen the letters and manuscripts of Newton." Leibnitz replied to this charge in the Leipsic Journal, without any asperity, simply stating himself to have been, as well as Newton, the inventor.

In the year 1705, on the publication of Newton's Quadrature of Curves, the same journalists insinuated, though with politeness and ambiguity, that Newton had been led to the notion of fluxions by the differentials of Leibnitz; just as Honoratus Fabri had been led to substitute the idea of progressive motion, for the indivisibles of Cavalleri. A charge so entirely unfounded, could not but call forth the indignation of Newton and his friends. In that indignation they were perfectly justifiable. But when the passions are heated, the injustice on one side is generally retaliated by an equal piece of injustice on the other. Accordingly, Keill, who undertook the defence of Newton's claims, instead of endeavoring to establish the priority of his discoveries by an appeal to facts and to dates that could be accurately ascertained; undertook to prove that the

communications of Newton to Leibnitz were sufficient to put the latter in possession of the principles of the new analysis, after which he had only to substitute the notion of differentials for that of fluxions. But in support of these charges he had nothing to offer but equivocal facts, and overstrained arguments, capable of convincing those only who were already disposed to believe. They were accordingly received as accurate in England; rejected as absurd in Germany, and read without conviction by the mathematicians of France and Italy.

Leibnitz complained of Keill's statements to the Royal Society of London, who declined giving any opinion; but appointed a committee of its members to draw up a full and detailed report of all the communications which had passed between Newton and Leibnitz, or their friends, on subjects connected with the new analysis, from the time of Collins and Oldenburgh, to the date of Keill's letter to Sir Hans Sloane in 1711, of which Leibnitz had complained. This report forms what is called the *Commercium Epistolicum*. It was published the year following by order of the Royal Society; and, though in the main just and fair, seemed rather to lean to the side of their own president. Leibnitz complained of this publication, and alleged, that though nothing was inserted which was not contained in the original epistles, yet certain passages were suppressed which were favorable to his pretensions. He threatened an answer, but it never appeared.—Some notes were added to the *Commercium Epistolicum*, which contained a good deal of asperity, and the review of this book inserted in the Philosophical Transactions for 1715, is still more liable to the same censure.

In the year 1713, a paragraph was circulated among the mathematicians of Europe, written by John Bernoilli. It amounted to this: "That there is no reason to believe that the fluctionary calculus was invented before the differential."—Bernouilli was doubtless well acquainted with the subject; but he was too much connected with Leibnitz, and had contributed too much to the progress of the differential calculus, to be an impartial judge.

The German mathematicians injured their own cause by attempting to fix on Newton the charge of plagiarism, which could be so triumphantly refuted. As much were the English mathematicians to blame when they retorted the same charge upon Leibnitz. It was not indeed physically impossible that Leibnitz might have borrowed his calculus, as Newton undoubtedly preceded him; but the assertion is not supported by the slightest shadow of a proof.

We shall pass over the subsequent defiances which passed mutually between the English and Continental mathematicians, and the harsh and unmerited epithets which they reciprocally bestowed upon each other, to the eternal disgrace of both parties, and the humiliation of mathematical science. The dispute was of a nature not capable of being decided by reasoning. Had Newton made known the nature of the fluxionary calculus when it first occurred to him, his claim as a discoverer would have been universally allowed, and he would have conferred a boon of the greatest magnitude on mathematicians. Leibnitz may have been informed that Newton was in possession of an unknown calculus of the greatest power—this information may have set his invention at work, and his exertions were crowned by the discovery of the differential calculus. This discovery, as soon as made, he gave to the public; and, with the assistance of the Bernoullis, soon brought it to a considerable degree of perfection. This proceeding was much more calculated for the improvement of science than the morbid timidity of Newton. The algorithm of Leibnitz was better; his method was taken up by men of first rate abilities, and prosecuted with astonishing success. For it would be difficult to find a series of mathematicians to be compared with the two Bernoullis, Euler, and Lagrange, who devoted in succession their unrivalled powers to the improvement and extension of the differential and integral calculus.

In England, at or before the time of Newton, the number of profound mathematicians was great. Wallis, Brounker, Wren, James Gregory, Barrow, were his immediate predecessors or contemporaries. Newton himself stands unrivalled, as, perhaps, the greatest mathematical genius that ever existed. Of his successors, the most remarkable is Cotes, whose *Harmonia Mensurarum* appeared in 1722. It contained the method of finding the fluents of fractional expressions, greatly generalized, and highly improved, by means of a property of the circle discovered by himself, and justly reckoned among the most remarkable propositions in geometry. It is curious that this book, notwithstanding its merit, has never acquired, among English mathematicians, the popularity which it deserves, while, on the continent, it seems to be very little known.

Another very original and profound writer of this period, was Dr. Brook Taylor, who, in his *Method of Increments*, published in 1715, added a new branch to the analysis of variable quantity. A single analytical formula in his *Method of Increments* has conferred a celebrity on its author which very voluminous works have often failed to bestow. It is known by the name of *Taylor's theorem*, and expresses the value of any function of a variable quantity, in terms of the successive orders of increments, whether finite or infinitely small. It is, perhaps, without exception, the most comprehensive proposition in the whole range of mathematical science.

Maclaurin may be mentioned as another mathematician, who did credit to his country.

The mathematicians of Britain, and on the continent, though the algorithm used by each was different, yet kept pace with each other for a considerable time, except in one branch, the integration of differential or fluxional equations. In this the British mathematicians had fallen considerably behind. And the distance between them, and those on the continent, continued to increase in proportion to the number and importance of the questions, physical and mathematical, which depended upon these integrations.—The habit of reading only British mathematical works, produced at first by the admiration of Newton, and afterwards continued, in consequence of the difference of notation, prevented the British mathematicians from partaking in the pursuits of the mathematicians on the continent. Prodi-

gious improvements were made in Italy, France, and Germany, in which the natives of Great Britain had little or no share.

Other causes, perhaps, may have contributed to draw away our men of science from mathematical investigations. But by the middle of the last century, the race of British mathematicians, at one time so numerous and so splendid, was reduced to a very small number indeed. It is true that mathematics still continued to be cultivated in Cambridge: but they satisfied themselves with studying the Principia of Newton, and neglected or despised the splendid improvements and discoveries of the continental mathematicians.

A little after the middle of the last century, Mr. West was appointed to teach the mathematical class in the University of St. Andrews, in consequence of the illness of Professor Vilant. West possessed an uncommon mathematical genius, as is evident from the slightest inspection of his Elementary System of Geometry, which he published while a teacher. His mode of teaching seems to have been admirable, and he had the merit of infusing his own spirit into a number of young men who have contributed not a little to the recovery of that high rank in mathematical science which formerly belonged to the British mathematicians. The late Professor Playfair was not indeed the pupil of West, but he was his friend and contemporary, and both had been educated at the same university. It is not unlikely, therefore, that he may have been indebted for his passion for the science, to his intimacy with West. The late Sir John Leslie was a pupil of West, and indebted to him for all his mathematical knowledge. He was possessed of a true mathematical genius, and though not familiar with the general analytical methods which are now in constant use, yet his mathematical knowledge was respectable. Mr. Glennie was another of Mr. West's pupils. But the man who does the highest credit to Mr. West, is Mr. Ivory, who has raised himself to the highest rank as a mathematician; who has cultivated every branch of the higher calculus with the most complete success; who is critically acquainted with the whole history of mathematical discoveries, and is now universally admitted to be the first mathematician at present in Europe. Thus he has rescued Great Britain from the stigma affixed to her, of inferiority in mathematical skill to the mathematicians on the continent. Cambridge also of late years has produced different eminent mathematicians, the most celebrated of whom is Sir John Herschell.

It would be impossible, in this hasty sketch, to give the slightest idea of the prodigious improvements which have been made in mathematics during the progress of the last century. The task is an herculean one. It has been frequently attempted, but never yet executed. The individual best qualified for such a task, is Mr. Ivory. Were he to execute it, he would confer a boon of no ordinary magnitude upon science, and add a new wreath to those with which Great Britain is already encircled.

III. ALGEBRA.

The word *algebra* is Arabic, and is derived from *al*, the, and *jebr*, contortion. It was at first applied to a particular arithmetical rule, in which the terms were transposed. There is some reason for suspecting that what we now call algebra, originated in India. Diophantus, who lived about 150 years after the commencement of the Christian era, has left thirteen books of arithmetical questions, which are treated in a manner that may be considered as algebraic. The science was cultivated by the Arabians during the golden age of Mahomedan science. And a knowledge of it was first brought to Europe in the 13th century, by Leonardo, a merchant of Pisa, who having made many visits to the east, brought back with him a knowledge of algebra, on which he wrote a treatise in 1202, and another in 1228, both of which still remain in manuscript.

But the first book printed in Europe on algebra was that of Lucas de Borgo, a Franciscan, who, towards the end of the 15th century, traveled into the East, and acquired a knowledge of the principles of algebra. The characters employed by him are merely abbreviations of words. The letters *p* and *m* are used for *plus* and *minus*. And the rule is laid down, that in multiplication, plus into minus gives minus; but minus into minus gives plus. Thus algebra was originally merely an abbreviation of common language, applied to the solution of arithmethical problems.

The Indians and Arabians advanced as far as the solution of quadratic equations. Scipio Ferrei, professor of mathematics at Bologna, had, about the year 1508, found out a method of solving one of the cases of cubic equations; which he either concealed, or at least communicated only to a few of his scholars. One of these, Florido, trusting to this secret, challenged Tartalea of Brescia to contend with him in the solution of algebraic problems. Florido had at first the advantage; but Tartalea being a man of ingenuity, soon discovered his rule, and likewise another much more general, in consequence of which he came off at last victorious. By the report of this victory, the curiosity of Cardan was strongly excited. For though he was himself an accomplished mathematician, he had not been able to discover a method of solving equations higher than the second degree. By the most importunate solicitations he wrung from Tartalea the secret of his rules; binding himself at the same time, by the most solemn promise, never to divulge them. Though Tartalea did not communicate the demonstration, Cardan soon found it out, and extended it in a very ingenious and systematic manner to all cubic equations whatever.

Being thus possessed of an important discovery, which was partly his own, he forgot his promises to Tartalea, and published the whole in 1545, not concealing, however, what he owed to the latter. Thus was published the rule which still bears the name of Cardan; and which still marks a point in the progress of algebraic investigation, which all the efforts of succeeding analysts have hardly been able to pass.

Robert Recorde, an English mathematician, published, about the middle of the 16th century, the first English treatise on algebra, and he there introduced the same sign of equality which is now in use.

The properties of algebraic equations were discovered very slowly. Pelitorious, a French math-

ematician, in 1558, first observed that the root of an equation is the divisor of the last term. Bombelli soon after published a regular treatise on algebra, in which he considered with particular attention the irreducible case of Cardan's rule, and pointed out the method of solving problems falling under it by the trisection of an arch.

Vieta marks an era in algebraic analysis. He was a Frenchman, born at Fonterai, in Poiton, in the year 1540. Though maitre des requetes at Paris, a situation which employed much of his time, he found leisure to devote himself to algebra. He first employed letters to denote both the known and unknown quantities; so that it was with him that the language of algebra first became capable of expressing general truths, and acquired that extension which has since rendered it such a powerful instrument of investigation. He likewise discovered the relation between the roots of an equation of any degree, and the coefficients of its terms, though only when none of the terms are wanting, and when all the terms are positive. This general truth, which Vieta only saw imperfectly, was further extended, in 1669, by Albert Gerard. But it was first developed in all its generality by Harriot, whose discoveries in algebra have been so much extolled by Wallis.

These successive improvements brought algebra nearly to its present state, and prepared it for a step which was about to be taken by Descartes, and which forms one of the most important epochs in mathematical science. This was the application of algebraic analysis to define the nature, and investigate the properties of curve lines; and consequently to represent the notion of variable quantities. This invention opened up a vast field for succeeding mathematicians. The work in which it was contained, is a tract of no more than 106 quarto pages. Probably there is no book of the same size, which has conferred so great, and so well merited a reputation on its author.

It would be impossible, in the short space to which we are obliged to confine ourselves, to give even an idea of the improvements which algebra has received during the two centuries which have elapsed since Descartes applied it to the investigation of curves. It cannot be said that any new principle has been discovered; but its language and symbols have been perfected, and it has been applied with effect to almost every kind of physical investigation. Indeed, if we consider the calculus of infinitesimal quantities as merely an extension of algebra, as might very well be done, we may say with truth, that it now constitutes the grand instrument by means of which physical science is promoted.

OF OBSERVATION AND EXPERIMENT.

It was not to be expected that mankind should at first make any rapid progress in investigating the laws which regulate the changes that take place in the material world. The objects were too numerous, and too varied, and escaped his attention by their very regularity. Every where in the early ages of the world, we meet with descriptions of prodigies and wonders, while the regular operations of nature scarcely attracted attention. The method of investigating nature by observation and experiment was scarcely thought of, except by two individuals, who, by means of them, made some progress in mechanics and hydrostatics, and in astronomy; these were Archimedes and Hipparchus. The mechanical discoveries of Archimedes were slightly extended by Ctesibius and Hero, by Anthemius, and by Pappus; while the astronomical observations begun by Hipparchus, were continued by Ptolemy.

But at the revival of letters, in the 16th century, a spirit of observation and inquiry awoke, which nothing could damp, and men began to pry into the secrets of nature, by the way of experiment. Galileo, in Italy, and Gilbert, in England, especially the former, constitute remarkable examples of the successful investigation by experiment. But it was Francis Bacon, Lord Verulam, who first investigated the laws according to which such experimental investigations should be conducted, who pointed out the necessity of following these laws in all attempts to extend the physical sciences, and who foretold the brilliant success that would one day repay those who should adopt the methods which he pointed out. This he did in his *Novum Organum*, published in the early part of the 17th century.

Before laying down the rules to be followed in his new, or inductive process, Bacon enumerated the causes of error, which he divided into four sets, and distinguished, according to the fashion of the times, by the following fanciful, but expressive names:—

Idols of the tribe,
Idols of the den,
Idols of the forum,
Idols of the theatre.

The *idols of the tribe* are the causes of error, founded on human nature in general. Thus all men have a propensity to find in nature a greater degree of order, simplicity, and regularity, than is actually indicated by observation. This propensity, usually distinguished by the title of *spirit of system*, is one of the greatest enemies to its progress that science has to struggle with.

The *idols of the den* are those that spring from the peculiar character of the individual. Each individual, according to Bacon, has his own dark cave or den, into which the light is imperfectly admitted, and in the obscurity of which an idol lurks, at whose shrine the truth is often sacrificed. Some minds are best adapted to catch the differences, others the resemblances of things.—Some proceed too rapidly, others too slowly. Almost every person has acquired a partiality for some branch of science, to which he is prone to fashion and force every other.

The *idols of the forum* are those which arise out of the intercourse of society, and especially from language, by means of which men communicate with each other. It is well known that words, in some measure, govern thought, and that we cannot think accurately unless we are able to express ourselves accurately. The same word does not convey the same idea to different persons. Hence many disputes are merely verbal, though the disputants may not be aware of the circumstance.

The *idols of the theatre* are the deceptions which have taken their rise from the systems of different schools of philosophy. These errors af-

fected the philosophy of the ancients more than that of the moderns. But they are not yet without their effect, and often act powerfully upon individuals without their being aware of their effect.

After an historical view of science from its dawn among the Greeks to his own time, and pointing out the little progress which it had made, in consequence of the improper way in which it had been cultivated, Bacon proceeds, in his second book, to point out the true way of advancing science by *induction*.

The first object ought to be to prepare a history of the phenomena to be explained, in all their modifications and varieties. This history is to comprehend not only all such facts as spontaneously offer themselves, but all the experiments instituted for the sake of discovery, or for any of the purposes of the useful arts. It ought to be composed with great care; the facts should be accurately related and distinctly arranged—their authenticity carefully ascertained, and those that are doubtful should be marked as uncertain, with the grounds for the judgement formed. This record of facts Bacon calls *natural history*.

The next object is a comparison of the different facts, to find out the cause of the *phenomenon*.

The method of induction here laid down is applicable to all investigations where experience is the guide, whether in the moral or natural world.

It is obvious that all facts, even supposing them truly and accurately recorded, are not of equal value in the discovery of truth. Some of them show the thing sought for in its highest degree, others in its lowest; some show it simple and uncombined, while others are confused with a variety of circumstances. Some facts are easily interpreted, others are very obscure, and are understood only in consequence of the light thrown on them by the former. This led Bacon to consider the comparative value of facts as means of discovery. He enumerates twenty-seven different species; but we shall satisfy ourselves here with noticing a few of the most important of them.

1. *Instantiæ solitariæ* are examples of the same quality existing in two bodies, which have nothing else in common; or of a quality differing in two bodies, which are in all other respects the same.

2. The *instantiæ migrantes* exhibit some nature or property of bodies passing from one condition to another, either from less to greater, or from greater to less. Thus, glass while entire is colorless, but becomes white when reduced to powder.

3. The *instantiæ ostensivæ* show some particular nature in its highest state of power or energy. In this way the thermometer shows the expansive power of heat, and the barometer the weight of air.

4. The *instantiæ analogicæ* consists of facts between which an analogy or resemblance is visible in some particulars, notwithstanding great diversity in all the rest. Such are the telescope and microscope in works of art, compared with the eye in the works of nature.

5. The *instantiæ crucis* is the division of this experimental logic which is the most frequently resorted to in the practice of inductive investigation. When in such an investigation, the understanding is, as it were, placed in equilibrio between two or more causes, each of which accounts equally well for the appearances, as far as they are known, nothing remains but to look out for a fact which can be explained by the one of these causes, and not by the other. If such a fact can be found, the uncertainty is removed, and the true cause becomes apparent. Such facts perform the office of a *cross*, erected at the meeting of two roads, to direct the traveler which way he is to go. On this account, Bacon gave them the name of *instantiæ crucis*. Suppose it were inquired into why metals become heavier when calcined, various explanations might be conceived. But the experimentum crucis of Lavoisier removed the ambiguity. He enclosed a quantity of tin in a large glass vessel, which was hermetically sealed. Heat being then applied, the tin melted and was partly calcined. The process being finished, the weight of the glass and its contents were found unchanged. But the glass being opened, a quantity of air rushed in, amounting in weight to ten grains; and the tin was found to have increased in weight to ten grains. It was obvious from this, that by the calcination of the tin a portion of the air had been absorbed, which had occasioned the increase of the weight.

In cases where an experimentum crucis cannot be resorted to, there is often a great want of conclusive evidence. This is the case in agriculture, in medicine, in political economy, &c. To make one experiment similar to another in all respects but one, is what the *experimentum crucis*, and the principle of induction, in general, requires.—But this, in the sciences just named, can seldom be accomplished. Hence the great difficulty of separating the causes, and allotting to each its due proportion of the effect. Men deceive themselves in consequence of this continually, and think they are reasoning from fact and experience, when, in reality, they are drawing their conclusions from a mixture of truth and falsehood.—Facts so incorrectly apprehended only serve to render error more incorrigible.

Of the twenty-seven classes into which *instantiæ* are arranged by Bacon, fifteen address themselves immediately to the understanding; five serve to correct or inform the senses; and seven to direct the hand in raising the superstructure of art on the foundation of science. The examples which we have selected, are from the first of these divisions. The other two are of inferior importance, and may be omitted in this imperfect summary.

Such are the rules laid down by Bacon for prosecuting the sciences by induction. The effects which were ultimately produced by the Novum Organum must have been very great. It may be questioned, indeed, whether those who have contributed most effectually to the advancement of the sciences, have rigidly adhered to Bacon's rules. And, in general, such a rigid adherence is unnecessary; because so much assistance can, in general, be derived from what knowledge has been already acquired, that a rigid natural historical detail of all the phenomena becomes unnecessary. It was only in the infancy of science that such details were requisite. Boyle often draws them up in his inquiries into the cause of various phenomena, and his investigations were

of considerable use in forwarding those branches of science which he cultivated. Bacon also was mistaken in conceiving that, by investigation, mankind may become acquainted with the *essences* of the powers and qualities residing in bodies. So far as science has hitherto advanced, no one essence has been discovered, either as to matter or as to any of its more extensive modifications. Thus we are still in doubt whether heat and electricity be qualities or substances. Yet we have discovered many important properties or laws, by means of which heat and electricity, whether properties or substances, are regulated.—And from this knowledge, probably, we derive as much advantage as could be obtained from a complete knowledge of their essence.

Such are the two methods of advancing science. By *experiment* or *observation* all the new facts in every science are acquired. By the application of mathematical reasoning to these facts they are reduced to the requisite simplicity, and the general principles which regulate every particular science determined. Let us now endeavor to trace the progress which has been made in the different physical sciences since these two powerful means of advancement were fairly applied to them.

I. MECHANICS.

Stevinus, an engineer in the Lower Countries, is the first person who passed beyond the point at which the ancients had stopped, by determining accurately the force necessary to sustain a body on a plane inclined at any angle to the horizon. This knowledge he seems to have deduced from the fact, that a chain laid on an inclined plane, with a part of it hanging over at top in a perpendicular line, will be in equilibrio if the two ends of the chain reach down exactly to the same level. The first appearance of Stevinus's solution of this problem was in the year 1585. His works, as we now have them, were collected after his death by his countryman, Albert Gerard, and published at Leyden in 1634.

But the man to whom mechanics is indebted for the first great steps which it made in advance is Galileo, who was born at Pisa in the year 1564, and who is, perhaps, the most remarkable man who appeared in that age, so prolific in men of first-rate genius. In 1592 he published a treatise, *Della Scienza Mechanica*, in which he has given the theory not only of the lever, but also of the inclined plane and screw, and in which he laid down this general proposition, that small weights are able to move large ones only by a great increase of velocity, or that weights are in equilibrio when the weight of each multiplied into its velocity is the same.

While a student at Pisa, he had made experiments on falling bodies, and discovered the fact that light and heavy bodies fall to the ground in the same time, making allowance for the resistance of the air. From observing the vibrations of the lamps in the cathedral he had come to this very important conclusion, that great and small vibrations of the pendulum are performed in the same time, and that this time depends on the length of the pendulum.

These experiments drew upon him the displeasure of his masters, who were offended that he should consult nature and experiment instead of Aristotle, and their commentaries on the dogmas of the Grecian sage. This was the origin of those persecutions, proceeding from a mixture of bigotry and envy, with which he continued to be harassed throughout the rest of his life.

By means of the inclined plane he succeeded in demonstrating that the motion of a falling body, is a uniformly accelerated motion. His next step was to determine the path of a heavy body, when obliquely projected. He showed this path to be a parabola. The theory of the inclined plane showed that if a circle be placed vertically, the chords of the different arches terminating in the lowest point of the circle are all descended through in the same space of time. But when Galileo applied this to account for great and small vibrations of a pendulum being performed in the same time, he fell into an error, which was first completely rectified by Huygens.

In the list of the mechanical discoveries of Galileo, may be placed the knowledge of the existence of the *law of continuity*, and the employment of it as a principle in his reasonings on the phenomena of motion.

Torricelli, the pupil and friend of Galileo, discovered a remarkable property of the centre of gravity, and a general principle with respect to the equilibrium of bodies. It is this: If there be any number of heavy bodies connected together, and so circumstanced, that by their motion their centre of gravity can neither ascend nor descend, these bodies will remain at rest.

Descartes, whose reputation was so great, and his pretensions so high, likewise treated of motion; but in general his opinions were so erroneous or unsound, that in the present rapid sketch they are not entitled to notice.

The laws which regulate the collision of bodies remained unknown, till they were recommended by the Royal Society to the particular attention of its members. Three papers soon appeared, in which these laws were correctly laid down, though no one of the authors had any knowledge of the conclusions obtained by the other two. The first of these was read to the society, in November, 1668, by Dr. Wallis of Oxford; the next by Sir Christopher Wren, in the month following; and the third by Huygens, in January, 1669. The equality of action and re-action, and the maxim that the same force communicates to different bodies velocities which are inversely as their masses, are the principles on which these investigations are founded.

Huygens was the first person who explained the true relation between the length of the pendulum, and the time of its last vibrations, and who gave a rule by which the time of the rectilinear descent through a line equal in length to the pendulum, might from thence be deduced. He next applied the pendulum to regulate the motion of a clock, and gave an account of its construction, and the principles of it in his Horologium Oscillatorium, about the year 1670, though the date of the investigation goes as far back as 1656. Lastly, he showed how to correct the imperfection of a pendulum, by making it to vibrate between cycloidal cheeks, in consequence of which its vibrations, whether great or small, become precisely equal.

The appearance of the *Principia* of Newton, in 1687, effected an almost total revolution in mechanics, by giving new powers and new directions to its researches. The composition of forces was treated independently of the composition of motion. From the equality of action and reaction, it was inferred, that the state of the centre of gravity of any system of bodies is not changed by the action of these bodies on each other.—From this it follows, that the quantity of motion existing in nature, when estimated in any given direction, continues always of the same amount.

But the reduction of questions concerning force and motion, to questions of pure geometry, and the mensuration of mechanical action by its nascent effects, constitute the great glory of the Principia. A transition was there made from the consideration of forces acting at stated intervals, to that of forces acting continually; and from forces constant in quantity and direction, to those that converge to a point, and vary as any function of the distance from that point; the proportionality of the areas described about the centre of force, to the time of their description; the equality of the velocities generated in descending through the same space by whatever rout; the relations between the squares of the velocities produced or extinguished; and the sum of the accelerating or retarding forces, computed with a reference not to the times during which, but to the distance over which they have acted; may be mentioned as a few of the dynamical and mechanical discoveries contained in that immortal work.

Leibnitz, the rival and antagonist of Newton, made some improvements in mechanical science, which though not capable of being compared with the profound discoveries of Newton, yet require to be noticed. Leibnitz seems to have been the first who announced in general terms the *principle of a sufficient reason.* This principle, however, was not new, for it had been assumed by Archimedes, and employed by Galileo. Stript of the metaphysical garb in which it was placed by Leibnitz, it seems nothing more, than that nothing exists in any state, without a reason determining it to be in that state rather than in any other. This principle, though true in itself in a limited sense, was rather brought into discredit by the way in which Leibnitz employed it. Thus he inferred from it, that two particles of matter cannot possess exactly the same properties; for if they did, the Supreme Being could have no reason for employing the one more than the other, and consequently both would be of necessity rejected—as if we were capable of judging in what way motives act upon the mind of the Deity, and—as if position might not be a sufficient motive for employing one particle rather than another, supposing both possessed of exactly the same properties.

Another principle brought into view by Leibnitz, was *the law of Continuity*—according to which, nothing passes from one state to another, without passing through all the intermediate states. Though Leibnitz considers himself as the first who pointed out this law, it is but fair to state that it was distinctly laid down by Galileo, who does not claim it as his own, but ascribes its discovery to Plato. This principle like the last, Leibnitz and his followers carried to a blamable excess. Thus John Bernoulli was induced by it to deny the existence of hard bodies altogether; because in the collision of such bodies a finite change must take place in an instant, which, according to the principles of the law of continuity is impossible. We can obviate the objection of Bernoulli without refusing, as Maclaurin does, to admit the law of continuity, by admitting that the hard bodies begin to act on each other before they come into actual contact.

The last mechanical improvement of Leibnitz introduced a controversy into mathematics, which was discussed by the most eminent mathematicians of Europe, for more than thirty years, with great keenness, and not a little virulence; though neither side was able to produce any change of opinion in their antagonists. Leibnitz, in 1686, announced in the Leipsic Journal *the demonstration of a great error committed by Descartes and others, in estimating the force of moving bodies.* In this paper he endeavors to show that the *force* of a moving body is not proportional to its velocity simply, but to the *square of its velocity.* And he supported this new doctrine by very plausible reasoning. A body projected upwards against gravity, with a double velocity, ascends four times the hight; with a triple velocity, to nine times the hight, and so on—the hight ascended being always as the square of the velocity. Such was the reasoning, sufficiently simple and satisfactory.

The subject was soon taken up keenly, and the world of science was divided into two parties. The mathematicians of Germany, Holland, and Italy, adopted the opinion of Leibnitz; those of Great Britain the old opinion, that the force is proportioned simply to the velocity; while those of France were divided between the two opinions. Maclaurin, Stirling, Desaguliers, Jurin, Clarke, and Mairan, defended the old opinion; while Bernoulli, Hermann, Poleni, 'S Gravesende, and Muchenbroek, supported the opinion of Leibnitz.

What may appear at first sight singular in this dispute, is, that the two parties who adopted such different measures of force, when any mechanical problem was proposed concerning the action of bodies, whether at rest or in motion, resolved it in the same manner, and arrived exactly at the same conclusions. It is clear from this, that their ideas or opinions exactly coincided. In reality, the two parties advanced positions not inconsistent with each other; and both therefore were true. This was pointed out by D'Alembert in his *Dynamique,* published in 1743.

We may measure the *force* of one moving body, by its effect upon another moving body. Hence there is no doubt that the forces of such bodies are as the quantities of matter multiplied into the velocities; because it is well known, that the forces of bodies in which these products are equal, if opposed, destroy each other. If we employ this measure, it is evident that the forces vary not as the squares, but simply as the velocities.

When a moving body is opposed by pressure, or a resistance like that of gravity, the quantity of such resistance required to extinguish the motion must serve to measure the force of the body. But there are two ways of computing the amount of these retarding forces, which lead to different results; both of them just, and neither of them to be assumed to the exclusion of the other. Sup-

pose a body to be projected perpendicularly upwards, in a direction opposite to that of gravity, we may either inquire into the retardation which gravity produces during a *given time*, or while the body is moving over a *given space*. We may inquire *how long* the motion will continue, or *how far* it will carry the body before it be entirely exhausted. If we employ the first of these for the measure of the force of a body, that force must be proportional to the velocity; for to this the time is manifestly proportional. If we employ the second, namely, the length of the line which the moving body describes, as the measure of the force, then it must be as the square of the velocity; because to that quantity the length of the line is known to be proportional.

Thus we obtain two values of the force; the one proportional to the velocity, the other to the square of the velocity. Who does not perceive, that the reason of this apparent inconsistency is the different meaning applied to the term *force* in the two cases?

So far as general principles are concerned, mechanics were nearly brought to a state of perfection by Newton and the mathematicians of his time. Perhaps the principle of the *conservation of living forces*, introduced by John Bernoulli, ought to be mentioned.

To understand what is meant by this principle, it may be requisite to observe, that to mere pressure Leibnitz gave the name of *vis mortua*, or *dead force*, and to the force of moving bodies the name of *vis viva* or *living force*. By the *conservation of living forces*, Bernoulli meant that permanence, through all the gradual changes of any system of connected bodies, in the aggregate of the products of their masses into the square of their velocities. It abridges the solution of various problems, and was adopted by Daniel Bernoulli, as the basis of his theory of hydrodynamics, published in 1738.

The task of composing a treatise on Dynamics, full and original in every part, was taken up by Euler. He bestowed upon it all the pains and all the resources of his penetrating genius. It appeared in 1736, in two quarto volumes, entitled *Mechanics, or the Science of Motion*, and contained the most elaborate and complete body of analytical investigation that had hitherto appeared.

D'Alembert published his Dynamics in 1743, and founded the whole of his reasoning upon a very simple general principle. In every system of bodies acting mutually, their several movements, at any instant of time, may be decomposed into two portions, one which is retained in the next instant, and the other spent; and since an equilibrium must obtain among the lost motions, an expression is thus obtained for the motions which are preserved. The most intricate questions in dynamics were thus reduced to mere statical problems, and solved constantly in the same easy and uniform way. Maclaurin's method of expounding forces by co-ordinates, facilitated still further the application of this principle, which D'Alembert in 1744 and 1752 extended also to hydrodynamics.

Segner's theory of the motion of *tops* deserves also to be mentioned. It was published in 1755. He shows that every body having a determinate figure, which after combined impulsions is abandoned in free space, will, besides its progressive motion, perform simultaneously and without the smallest interference, a constant and uniform revolution about each of three principal axes, mutually perpendicular and passing through the centre of gravity. These axes of rotation possess some curious properties, which were afterwards investigated by Euler.

In 1788, Lagrange, by combining the principle of D'Alembert with that of virtual velocities, converted the whole into an absolutely analytical science. He referred the efforts of every particle of a moving system to three mutual perpendiculars, and thence derived three several differential equations, which, being integrated, would give the final solution of the problem. But no general formula for integrating such equations has been hitherto discovered.

Such is a sketch of the gradual progress of the mathematical investigation of mechanics. To enumerate the application of these general principles to the solution of particular problems—to point out the gradual improvements which have been introduced into the construction of mechanical engines, and the immense advantages which have resulted from these improvements, especially in this country, where they have been carried to the greatest extent, would require a treatise of no ordinary length, and would be quite incompatible with the limits within which we are under the necessity of confining ourselves.

ASTRONOMY.

The ancients discovered the planets, and gave them names, and noticed their motions, at a very early period. The motions of the sun and moon could not fail to attract their attention, and the changes in the seasons which depended upon these motions. The phenomena of the eclipses of these luminaries, viewed with such apprehension by the common people, necessarily attracted the attention of the first observers of the heavenly bodies: they soon observed a certain regularity in these phenomena, and became, in consequence, capable of predicting them. All these appearances could not be familiar to them without some speculations about the motions of the heavenly bodies.

The stars appeared as so many luminous points fixed in the heavenly sphere, having the earth for a centre, and revolving on an axis having that earth for a centre in the space of twenty-four hours. All the stars were found not to partake of this diurnal motion in the same degree; some were carried slowly to the east, and their paths, after a certain interval of time, returned upon themselves. The astronomers of the Alexandrian school set themselves to ascertain the general laws of these motions. This could not be well done without a hypothesis; and the simplest was, that the planets move eastwards in circles, and at a uniform rate.

It was soon found, however, that the motion eastward was not uniform. The planet began to move slower and slower, and, at last, became stationary. It then acquired a motion in a contrary direction, and, after proceeding for a certain time westwards it became stationary, and then moved

eastwards as at first. These motions were not easily reconciled with a uniform circular motion. The explanation, however, was attempted by Apollonius Pergæus. He conceived that, in the circumference of a circle having the earth for its centre, there moved the centre of another circle, in the circumference of which the planet actually revolved. The first of these circles was called the *deferent*, and the second, the *epicycle;* the motion in the circumference of each was supposed uniform. Lastly, it was conceived that the motion of the centre of the epicycle in the circumference of the deferent, and of the planet in the epicycle, were in opposite directions; the first being towards the east, and the second towards the west. In this way, the alterations from progressive to retrograde, with the intermediate stationary points, were readily explained; and Apollonius carried his investigation so far as to determine the ratio between the radius of the deferent and that of the epicycle. from knowing the stations and retrogradations of any particular planet.

Hipparchus, the greatest astronomer among the ancients, discovered the inequality of the sun's apparent motion round the earth. To express this irregularity, he imagined an epicycle of a small radius, with its centre moving uniformly in the circumference of a large circle having the earth for its centre, while the sun revolved uniformly in the circumference of the small circle, but in a contrary direction.

As other irregularities in the motions of the moon and planets were observed, other epicycles were introduced; and Ptolemy, in his *Almagest*, enumerated all which then appeared necessary, and assigned to them such dimensions as enabled them to express the phenomena with accuracy.

By this contrivance the system of the heavens became extremely complicated. But it had the advantage of subjecting all the motions of the sun, moon and planets, very readily to a geometrical construction, or an arithmetical calculation of no great difficulty. Hence the predictions of astronomical phenomena, the calculation of tables, and the comparison of these tables with observations, became easy; and upon this the progress of the science depends. We have no evidence that the ancient astronomers ever considered the epicycles and deferents, which they employed in their systems, as having a physical existence, or as serving to represent these motions, they merely employed them to enable them to calculate the apparent motions of the heavenly bodies.

When Europe began to awake from the lethargy of the dark ages, astronomy was the first of the sciences which drew the attention of men of science. Purbach and Regiomontanus contributed most towards its advancement during the 15th century. Purbach resided at Vienna, under the patronage of the emperor Frederick the Third, and devoted himself to astronomical observations. He published an edition of the *Almagest;* and though he neither understood Greek nor Arabic, yet his knowledge of the subject enabled him to make it much more perfect than any former translation. Regiomontanus was a pupil of Purbach, and became much more celebrated than his master.

Copernicus, who had the merit of first divining the true system of the universe, was born at Thorn, in the year 1473, studied at Cracow, and ultimately went into the church. A decided taste for astronomy led him, when very young, to the study of that science. It occurred to him, at a very early period, to consider what effect the motions of the heavenly bodies would have upon a spectator, transferring that motion to the objects observed, but ascribing to it an opposite direction. It became immediately obvious, that the rotation of the earth, on its axis from west to east, would produce the apparent motion of the heavens from east to west.

In considering the objections which might be made to the system of the earth's motion he reasons soundly, though he was not aware of the full force of his own argument. Ptolemy had alleged, that if the earth were to revolve on its axis, the violence of the motion would be sufficient to tear it in pieces, and dissipate the parts. Why, says Copernicus, was he not more alarmed for the safety of the heavens, if the diurnal motion be ascribed to them, as their motion must be more rapid in proportion as their magnitude is greater?

We need not mention that Copernicus placed the sun in the centre, and taught that all the planets moved round that luminary in orbits nearly circular. The moon revolved round the earth, and the apparent diurnal revolution of the heavens from east to west was owing to the real diurnal revolution of the earth from west to east. The first edition of the *Astronomia Instaurata*, in which these doctrines appeared, was dedicated to the pope, and was published in 1543 a few days before the death of the author.

After Copernicus, Tycho Brahe was the most distinguished astronomer of the sixteenth century. An eclipse of the sun, which he witnessed in 1560, when he was a young man, in consequence of the exactness with which it answered the prediction, inspired him with a veneration for the science, and an anxious desire to become acquainted with it. Unfortunately for his progress, he belonged to a noble family in Denmark —a class of men entitled, by their rank, to disclaim the pursuit of knowledge, and extremely jealous of the privilege of ignorance. But his enthusiasm enabled him to break through the trammels of his order. He even acquired the patronage of the king of Denmark, and was able, in consequence, to erect an observatory in the island of Huena, supplied with far better instruments than had ever yet been applied to astronomical observations. Tycho, by means of them could measure angles to ten seconds, which was sixty times the accuracy of Ptolemy, or of any instruments that had belonged to the school of Alexandria.

His first object was a catalogue of the stars, which he was anxious to make more accurate than that of Hipparchus and Ptolemy. The great difficulty in executing this task, proceeded from the want of an easy method of determining the distance of one heavenly body east or west of another. The distance north or south was easily determined by the common method of meridian altitudes. For the equator is a plane, which, for any given place on the earth's surface, always retains the same position. But no plane passing through the poles retains a fixed position with respect to an observer. The natural substitute is the

measure of time. But this was wanting both to the Greek astronomers and to Tycho. Hipparchus and Ptolemy determined the longitude of the fixed stars, by referring their places to those of the moon. Thus depending on the most irregular of the heavenly bodies for determining the position of the most fixed. Tycho made use of the planet Venus, instead of the moon; and his method, though more tedious, was more accurate than that of the Greek astronomers. His catalogue contained the place of 777 stars.

The irregularities of the moon's motion were the next subject of inquiry. The ancients had discovered the inequality of this planet, depending on the eccentricity of the centre, and now called *the equation of the centre*. Ptolemy had added the knowledge of another inequality in the moon's motion, to which the name of *evection* has been given, causing an increase of the former equation at the quarters, and a diminution of it at the times of new and full moon. Tycho discovered another inequality, which is greatest at the octants, and depends upon the difference between the longitude of the moon and of the sun. A fourth inequality, depending wholly on the sun's place, was known to Tycho, but included among the sun's equations. Besides, these observations made him acquainted with the changes in the inclination of the plane of the moon's orbit; and, lastly with the irregular motion of the nodes, which, instead of being always retrograde at the same rate, are subject to change that rate, and even to become progressive, according to their situation in respect to the sun. These constitute all the irregularities in the moon's motion known before the development of the theory of gravitation, and all, except the two first, were discovered by Tycho.

The atmospherical refraction, by which the heavenly bodies are made to appear more elevated above the horizon than they really are, was suspected, indeed, before the time of Tycho; but not certainly known to exist. He first became acquainted with it by finding that the latitude of his observatory, as determined by observations at the solstices, and from observations of the greatest and least altitudes of the circumpolar stars, always differed about four minutes. He supposed the effect of refraction to be 34′ at the horizon, and to diminish from thence upwards to 45°, where it ceases altogether. This last opinion is erroneous; but at 45° it is less than 1′, a quantity probably not discernible by his instruments in the altitudes measured. He contrived an instrument, on purpose to make the refraction distinctly visible. It was an equatorial circle of ten feet diameter, turning on an axis parallel to that of the earth. With the sights of this equatorial, he followed the sun on the day of the summer solstice, and found that, as it descended towards the horizon, it rose above the plane of the instrument.—At its setting, the sun was raised above the horizon by more than its own diameter.

The comet of 1570 was observed by Tycho, and gave rise to a new theory of these bodies. Its parallax was 20′, showing that it was three times farther off than the moon. He considered comets, in consequence, as bodies placed far beyond the range of our atmosphere, and moving round the sun. His observations on the new star of 1572, deserve also to be noticed. It appeared in Cassiopeia, on the 7th of November, all at once, and surpassed all the stars in splendor, being scarcely inferior to Venus herself. In the month of January, 1573, it was rather less than Jupiter. From this time it was constantly diminishing in splendor, and disappeared altogether in the month of March, 1574. Pliny informs us that it was the appearance of a new star which led Hipparchus to think of making a catalogue of the stars.

Tycho, notwithstanding his merit as an observer, could not prevail upon himself to adopt the Copernican system of the motion of the earth round the sun. He made the sun move round the earth, while it was at the same time the centre of the planetary motion.

Kepler followed Tycho. He was born in 1570, and at an early age applied himself to the study of the heavens. His first discussions related to astronomical refraction, and to the calculation of eclipses.

He observed that the orbits of the planets are in planes passing through the sun, and that of consequence the lines of their nodes all intersect in the centre of that luminary. The opposition of the planets, or their places when they pass the meridian at midnight, offer the most favorable opportunities for observing them, both because they are at that time nearest the earth, and because their places seen from thence is the same as if they were seen from the sun. But the true time of opposition had hitherto been mistaken by astronomers, who held it to be at the moment when the apparent place of the planet was opposite to the mean place of the sun. It ought to have been when the apparent places of both were opposed to each other. This correction was made by Kepler, and though violently opposed by Tycho, was finally acquiesced in.

Having undertaken to examine the orbit of Mars, in which the irregularities are most considerable, Kepler discovered by comparing together seven oppositions of that planet, that its orbit is elliptical; that the sun is placed in one of the foci; and that there is no point round which the angular motion is uniform. In the pursuit of this inquiry he found that the same thing is true of the earth's orbit round the sun. It was reasonable to conclude by analogy, that the orbits of all the other planets are elliptical, having the sun in their common focus.

The industry and patience of Kepler, in these investigations, are almost incredible. Logarithms did not yet exist, so that arithmetical calculations were extremely laborious. The computation of every opposition of Mars filled ten folio pages; and Kepler repeated each calculation ten times. Seven oppositions thus calculated filled 700 folio pages. In such calculations the introduction of hypotheses was unavoidable. Kepler rejected them whenever they appeared erroneous, without hesitation, regretting merely the time which they had uselessly cost him. He began with hypotheses, and ended by rejecting every thing hypothetical.

Though the angular motion of the planets was not found to be uniform, it was discovered that a very simple law connected that motion with the rectilinear distance from the sun, the former being every where inversely as the square of the latter.

Hence it was easy to prove, that the area described by the line drawn from the planet to the sun increased at a uniform rate; and, therefore, that any two such areas were proportioned to the times in which they were described.

This was the second of Kepler's three famous laws; the third was not less remarkable. He was of opinion that some simple relation must exist between the periodical times of the planets and their distances from the sun. After an infinite number of trials, he discovered, to his great delight, that in any two planets the *squares* of the *times* of the revolution are as the *cubes* of their *mean distances* from the sun.

It is humiliating to be obliged to state, that these great discoveries were at first underrated by astronomers, and that they even reproached Kepler for making them; because, in calculating the place of a planet, they introduced a problem too difficult to be resolved by elementary geometry.

While Kepler was thus perfecting the theory of the planetary motions, Galileo had constructed a telescope, and directed it to the moon. The appearance of that luminary, under the telescope, is now so well known, that we need not describe it. The effect produced upon the mind of Galileo may be easily conceived. He became satisfied of the resemblance between the surface of the moon and that of the earth. The telescope brought into view multitudes of fixed stars, which cannot be seen by the naked eye. In Jupiter he observed a large disc, approaching in size to the moon.—Near it he saw, for the first time, three luminous points in a straight line; two of them on one side, and one on the other. By observing them, however, night after night, he found these small stars to be four in number, and to be moons or satellites accompanying Jupiter, and revolving round him as the moon does round the earth.

In Saturn he saw one large disc, with two smaller ones very near it, and diametrically opposite, and always seen in the same places. But more powerful telescopes were wanting before these appearances could be interpreted.

The horned figure of Venus, and the gibbosity of Mars, added to the evidence of the Copernican system, and verified the conjecture of its author, who ventured to say, that if the sight were sufficiently powerful, we should see Mercury and Venus exhibiting phases similar to that of the moon.

These discoveries were probably the most splendid that ever fell to the lot of any individual.—In a more enlightened age they would have secured the gratitude and admiration of the whole scientific world. But Galileo had raised a host of enemies, by attempting to overturn the Aristotelian doctrines, and the church itself was roused to action, because it had staked its infallibility in support of dogmas which the discoveries and reasoning of Galileo had overturned. Galileo was twice brought before the inquisition; and a council of seven cardinals pronounced the following sentence: "That to maintain the sun to be immovable, and without local motion in the centre of the world, is an absurd proposition, false in philosophy, heretical in religion, and contrary to the testimony of Scripture. That it is equally absurd and false in philosophy to assert, that the earth is not unmovable in the centre of the world, and, considered, theologically, equally erroneous and heretical."

Here was an example, among many others that might be given, of a number of men who conceived that power was able to subdue truth. But the puny efforts of popes and cardinals are totally unable to stop the steady flow of human knowledge. But who pays any regard to it?* A promise was extorted from Galileo that he would not teach the doctrine of the earth's motion, either by speaking or writing. But this promise he did not keep. His third dialogue, published long afterwards, contains a full display of the beauty of the new system, and such an exposure of the inconsistencies of Ptolemy and Tycho, as completed the triumph of the new system.

In 1663, when he was seventy years of age, he was again brought before the inquisition, and forced solemnly to disavow his belief in the earth's motion. He was condemned to perpetual imprisonment; though the sentence was afterwards mitigated, and he was allowed to return to Florence. This sentence sank heavily into his mind; he never after either talked or wrote on the subject of astronomy. But no decisions of popes or inquisitors could stop the progress of truth. The opinion that the earth is one of the planets, and that, like the others, it moves round the sun in an elliptical orbit, became more and more prevalent, and was soon universally embraced.

The time was now come when the motions of the heavenly bodies were to be compared with the laws of motion, as known on the earth. For this comparison, and for the development of a new science, the sublimest that has hitherto been exposed to the eyes of mankind, the world is indebted to Newton. In the year 1666, when he was a very young man, he was driven from Cambridge by the plague. As he sat one day in his garden, musing on the nature of the mysterious force by which the phenomena at the earth's surface are so much regulated, he observed an apple fall from a tree. The thought occurred to him, since gravity is a tendency not confined to bodies on the very surface of the earth; but since it reaches to the tops of trees, and to the summits of the highest mountains, without its intensity suffering any sensible change, why may it not reach to a much greater distance, and even to the moon itself? And if so, may not the moon be retained in her orbit by gravity, and forced to describe a curve, like a projectile, on the surface of the earth? Here another consideration very naturally occurred. Though gravity be not sensibly weakened at small distances from the earth, yet it may be weakened at greater distances, and at the moon may be much less. To estimate the quantity of diminution, Newton seems to have reasoned thus: If the moon be retained in her orbit by the gravitation of the earth, it is probable that the planets likewise are carried round the sun, in consequence of a similar power directed to

* In the edition of Newton's Principia Mathematica by the Jesuits, Le Seur abd Jacquier, printed at Rome in the year 1742, there occurs at the beginning of the third book the following declaration, by the reverend editors: "Newton in this third book has assumed the hypothesis of the motion of the earth. The propositions of the author could not be explained without adopting that hypothesis. We were therefore forced, in consequence, to adopt the opinion of another person. But we acknowledge ourselves obedient to the decrees of the pope against the motion of the earth."

centre of that luminary. He proceeded, therefore, to inquire by what law the gravitation of the planets to the sun must diminish, in order to correspond with Kepler's third law.

Such an investigation would have been beyond the power of most mathematicians of that age; but Newton soon discovered that Kepler's law would require the force of gravity to diminish exactly as the square of the distance increased. The moon, therefore, being distant from the earth about sixty semi-diameters of the earth, the force of gravity at that distance must be reduced to the 3600th part of what it is at the earth's surface. Was the deflexion of the moon, then, from the tangent of her orbit in a second of time, just the 3600th part of the distance which a heavy body falls in a second at the earth's surface? This question could be precisely answered, supposing the moon's distance in feet known, and her angular velocity, or the time of her revolution in her orbit also known.

Being at a distance from books, he took the common estimate of the earth's circumference then in use; according to which, a degree was held equal to sixty miles. This being an erroneous supposition, the result of the calculation did not represent the force as adequate to produce the effect. Hence Newton concluded that some other cause than gravity must act on the moon, and laid aside, in consequence, all further speculations on the subject for the time.

Some years after, his attention was again called to the subject by a letter from Dr. Hooke, proposing, as a question, to determine the line in which a body let fall from a hight descends to the ground, taking into consideration the motion to the earth on its axis. This led him to resume the subject of the moon's motion, and the measure of a degree by Norwood having now furnished more exact data, he found that his calculation gave the precise quantity for the moon's momentary deflexion from the tangent of her orbit, which was deduced from astronomical observation. The moon, therefore, has a tendency to descend to the earth from the same cause that a stone at its surface has; and if the descent of a stone in a second be diminished in the ratio of 1 to 3600, it will give the quantity which the moon descends in a second below the tangent of her orbit. Thus is obtained an experimental proof that gravity decreases as the square of the distance increases. He had already found that the times of the planetary revolutions, supposing their orbits to be circular, led to the same conclusion. He now proceeded, with a view to the solution of Hooke's problem, to inquire what their orbits must be, supposing the centripetal force to be inversely as the square of the distance, and the initial force to be any whatever. On this subject, we are told, that he composed about a dozen propositions probably those at the beginning of the Principia.

After this noble opening it is very surprising that he again dropped the investigation, and was not induced to take it up again till several years after, when Dr. Halley paid him a visit at Cambridge, and prevailed upon him to renew and extend his researches.

He then found that the three laws of Kepler are direct consequences of the system of gravitation. He showed that they all followed from the law that the planets gravitate to the sun, with a force inversely as the square of the distances. It added much to this evidence that the observations of Cassini had proved the same laws to prevail among the satellites of Jupiter.

Did the principle which appears to unite the great bodies of the universe act only on these bodies? Did it reside merely in their centres, or was it a force common to all the particles of matter? It could hardly be doubted that this tendency was common to all the particles of matter. For the centres of the great bodies had no properties but those derived from the particles distributed around them. But the question admitted of being brought to a better test than mere abstract reasoning. The bodies between which this tendency had been observed to take place were all round bodies, and nearly spherical, and whether large or small, they seemed to gravitate towards each other, according to the same law. The planets gravitate to the sun, the moon to the earth, the satellites of Jupiter to that planet—and gravity, in all these cases, varies inversely as the square of the distances. It was, therefore, safe to infer that however small the bodies, provided they were round, they would gravitate to each other with forces varying inversely as the squares of the distances. It was probable, then, that gravity was the mutual tendency of all the particles of matter to each other. But this could not be concluded with certainty, till it was known whether great spherical bodies, composed of particles gravitating according to this law, would themselves gravitate according to the same.

This problem Newton undertook to solve. He reduced it to the quadrature of curves, and found, no doubt, with delight, that the law was the same for the sphere as for the particles which compose it. That the gravitation was directed to the centre of the sphere, and was, as the quantity of matter contained in it, divided by the square of the distance from the centre. Thus a complete expression was obtained for the law of gravity, involving both the conditions on which it must depend, the quantity of matter in the gravitating bodies, and the distance at which the bodies are placed. There could be no doubt that this tendency was always mutual, and there was no exception to the rule that action and re-action are equal. So that if a stone gravitates to the earth, the earth equally gravitates to the stone; or, in other words, the two bodies approach each other with velocities which are inversely as their quantities of matter.

Newton went further, and showed how the quantity of matter, and even the density of the planets, might be determined. In the way already explained, he was enabled to compare the intensity of the earth's gravitation to the sun, with that of the moon to the earth, each being measured by the momentary deflexion from the tangent to the small arch of its orbit. A more detailed investigation showed that the intensity of the central force in different orbits, is as the mean distance divided by the square of the periodic time. And the same intensity being also, as the quantities of matter divided by the squares of the distances, it follows that these two quotients are equal to each other; and that, therefore, the quantities of matter are as the mean distances divided

by the squares of the periodic time. Supposing, therefore, that the ratio of the mean distance of the sun from the earth, to the mean distance of the moon from the earth is given; as the ratio of their periodic times is also known, the ratio of the quantity of matter in the sun, to the quantity of matter in the earth, of consequence is found. And the same thing holds in all the planets which have satellites moving round them. Hence also their mean densities, or specific gravities, become known.

The *Principia Philosophæ Naturalis*, which contained all these discoveries, was published in the year 1687; and many years elapsed before it became generally known and understood.

The principle of gravity thus fully established, and its consequences developed, was not mentioned by Newton for the first time. Some curious references to it are found in the writings of the ancients; and Copernicus, Kepler, and Hooke entertained opinions respecting it approaching much nearer to accuracy. "I do not think," says Copernicus, "that gravity is any thing else than an *appetency* of the parts of the earth given by the providence of the Supreme Being, that by uniting together they may assume the form of a globe. It is probable that this same affection belongs to the sun, the moon, and the fixed stars, which are all of a round form."

Kepler, in his great work on the *Motions of Mars*, treats of gravity as a force acting naturally from planet to planet, and particularly from the earth to the moon. "If the moon and the earth were not retained by some animal or equivalent force, each in its orbit, the earth would ascend to the moon by a 54th part of the interval between them, while the moon moved over the remaining fifty-three parts; that is, supposing them of the same density." This passage displays a curious mixture of ignorance and knowledge respecting the planetary motions.

Hooke made a near approach to truth than any of his predecessors. In his attempt to prove the motion of the earth in 1674, he lays it down as a principle, that the heavenly bodies have an attraction towards their own centres, which extends to other bodies within the sphere of their activity. The force of gravity he considered as greatest nearest the body, though he could not determine the rate of variation. These were considerable advances, though his opinions were mixed with much error and much ignorance. Yet he was disingenuous enough, when Newton had determined the law according to which gravitation varies, to lay claim himself to the discovery.

Of all the physical principles that have been hitherto made known, there is none so fruitful in consequences as that of gravitation. The same sagacity that led to the discovery was necessary to trace its consequences.

The mutual gravitation of all bodies being admitted, it was evident that, while the planets were describing their orbits round the sun, they must mutually attract one another; and hence, in their revolutions some irregularities, from the description of equal areas in equal times, might be expected. But hitherto such irregularities had been observed only in the revolutions of the moon. This led Newton to inquire what the forces were, which, according to the laws just discovered, could produce the irregularities in question. The moon must be acted on not only by the earth, but also by the sun; and it was at once evident that the force which was sufficient to bend the orbit of the earth into the form of an ellipse, must have a sensible effect on the orbit of the moon. He showed that it is not the whole force which the sun exerts on the moon that disturbs her motion round the earth, but only the difference between that force and the force which the sun exerts on the earth. To obtain exact measures of the disturbing forces, he supposed the entire force of the sun on the moon to be resolved into two, of which one always passed through the centre of the earth, and the other was always parallel to the line joining the sun and earth, consequently to the direction of the force of the sun on the earth. The former of these forces being directed to the centre of the earth, does not prevent the moon from describing round the earth equal areas in equal times. But the effect of it on the whole is to diminish the gravitation of the moon to the earth about one 358th part, to increase her mean distance in the same proportion, and her angular motion by about a 179th. He proved, by a very subtle investigation, that these forces would not sensibly change the elliptical orbit of the moon, but that the orbit itself would be rendered movable; its longer axis having an angular and progressive motion, by which it advanced over a certain arc during each revolution of the moon. This afforded an explanation of the motion of the apsides of the lunar orbit, which had been observed to go forward at the rate of 3° 4′ nearly, during the time of the moon's revolution, in respect of the fixed stars.

But the exact quantity of the motion of the apsides did not correspond with the diminution of the moon's gravity, as above assigned. There was, therefore, a cloud overshadowing this part of the lunar theory, which was not dissipated till a greater advance in mathematical knowledge put it in the power of subsequent astronomers to investigate the subject completely.

The line of the lunar nodes had been observed to retrograde at the rate of 3° 10′ every day. Newton showed that the second of the forces into which the solar action was resolved, being exerted not in the plane of the moon's orbit, but in that of the ecliptic, inclined to the former, at an angle somewhat greater than five degrees, its effect must be to draw down the moon to the plane of the ecliptic, sooner than it would otherwise arrive at it; in consequence of which, the intersection of the two planes would approach, as it were, towards the moon, or move in a direction opposite to that of the moon's motion, or become retrograde. From the quantity of the solar force, and the inclination of the moon's orbit, Newton determined the mean quantity of this retrogradation, as well as the irregularities to which it is subject, and found both to agree, corresponding very accurately with observation.

The lunar inequality discovered by Tycho, and called by him the *Variation*, which consists in the alternate acceleration and retardation of the moon in each quarter of her revolution, was accurately determined from theory, such as it is found by observation. The same remark applies to the annual equation, which had been long con-

founded with the equation of time. It does not appear that Newton attempted an exact determination of the other inequalities of the lunar motion. He satisfied himself with the general truth, that the principle of the sun's disturbing force led to the supposition of inequalities of the same kind with those actually observed. The full knowledge of all these inequalities, and their exact accordance with theory, was reserved for a future period, when a more perfect state of the calculus enabled philosophers to investigate the whole subject.

The earth, in consequence of its rotation on its axis, is influenced by a centrifugal force, which must act most powerfully on the parts most distant from the axis. The amount of this centrifugal force is greatest at the equator; and being measured by the momentary recess of any point from the tangent, which was known from the earth's rotation, it could be compared with the force of gravity at the same place, measured in like manner by the descent of a heavy body in the first moment of its fall. Newton found that the centrifugal force at the equator is the 289th part of gravitation, diminishing continually as the cosine of the latitude, on going from the equator to the poles, where it vanishes altogether. From the combination of this force with that of gravity, it follows that the plumb line cannot tend exactly to the earth's centre, and that a true horizontal line, such as is drawn by leveling, if continued from either pole in the plane of a meridian all around the earth, would not be a circle but an ellipse, having its greatest axis in the plane of the equator, and its least in the direction of the axis of the earth's rotation. Now the surface of the ocean itself actually traces this level, as it extends from the equator to the poles. Hence it follows, that the figure of the earth is an oblate spheroid, or a solid generated by the revolution of the elliptic meridian about its shorter axis. To determine the proportion of the axis spheroid, Newton conceived that, if the waters at the pole and at the equator were to communicate by a canal through the interior of the earth, one branch reaching from the pole to the centre, and the other at right angles to it from the centre to the circumference of the equator, the water in this canal must be in equilibrio, or the weight of the fluid in the one branch just equal to that in the other. By a very suitable process of reasoning, he found that the length of the equatorial canal must be to that of the polar as 230 to 229. It was demonstrated afterwards by Maclaurin and Clairaut, that this is in fact the ratio of the two diameters of the earth, supposing its specific gravity to be homogeneous from circumference to the centre, so as to be infinitely great at the centre, then the difference between the two diameters would be minimum, and would amount only to one 578th part. Mr. Ivory has examined this subject with his usual sagacity and profound mathematical knowledge, and concludes that the true difference between the length of the two diameters is one 300th part. This determination, we may safely assume, as exceedingly near the truth.

From the figure of the earth thus determined, Newton showed that the intensity of gravity at any point of the surface, is inversely as the distance of that point from the centre. Its increase, therefore, in going from the equator to the poles, is as the square of the sine of the latitude, the same ratio in which the degrees of the meridian increase. As gravity diminishes in going from the poles to the equator, it follows that a pendulum of a given length would vibrate slower when carried from Europe to the torrid zone. This had been already verified by the observations of Varin and De Hayes, made at Cayenne and Martinique.

What is called the *precession of the equinoxes*, or the retrogradation of the equinoctial points, had been long known. Its rate had been found to amount nearly to 50″ annually, so as to complete an entire revolution of the heavens in 25,920 years. Nothing seemed more difficult to explain than this phenomenon—no preceding astronomer had even thrown out a conjecture on the subject. It was reserved for the sagacity of Newton. He was directed by a certain analogy between the precession of the equinoxes, and the retrogradation of the moon's nodes, a phenomenon to which his calculus had been already successfully applied. The spheroidal shell, or ring of matter which surrounds the earth in the direction of the equator, being one half above the plane of the ecliptic, and one half below, is subjected to the action of the solar force, the tendency of which is to make this ring turn on the line of its intersection with the ecliptic, so as ultimately to coincide with the plane of that circle. This would have happened long since if the earth had not turned on its axis. The effect of the ration of the spheroidal ring from west to east, at the same time that it is drawn down towards the plane of the eliptic, is to preserve the inclination of these two planes unchanged, but to make their intersection move in a direction opposite to that of the diurnal rotation, that is, from east to west, or contrary to the order of the signs; 10″ appeared to be the part of the effect due to the moon's attraction, and 40″ to the attraction of the sun.—How Newton obtained these numbers does not appear; his data seem scarcely sufficient for the purpose; yet as his results are correct, the probability is that he was in possession of data which he has not stated to his readers. The subject was completed at the interval of a century, by the exertions of Lagrange and Laplace.

Newton next turned his attention to the phenomena of the tides, the dependence of which on the sun and moon was sufficiently obvious to common observation. That the moon has the greatest effect in producing the tide, is evident from this, that high water always occurs at a place nearly at the time when the moon is in the same meridian, that the daily retardation of the tide is synchronous with the retardation of the moon in her diurnal revolution. It is equally evident that the sun is concerned in the tides, as the highest tides happen when the sun, moon, and earth are all three in a straight line, and lowest tides when the lines drawn from the sun and moon to the earth make right angles with each other.

Newton perceived that the waters revolving round the earth are nearly in a condition of a satellite revolving about its primary, and liable to the same kind of disturbance from a third body.—High water always takes place in the hem-

isphere where the moon is, and in the opposite hemisphere where the moon is not, nearly at the same time. Now if the action of the moon disturb the equilibrium of the ocean, just as the action of one planet disturbs the motion of a satellite moving round another, this is exactly what might be expected. It has been proved that the moon when in conjunction with the sun, had her gravitation to the earth diminished, and when, in opposition to the sun, has it diminished very nearly by the same quantity. For at the new moon the moon is drawn to the sun more than the earth is, and at the full moon the earth is drawn to the sun more than the moon is, and nearly by the same quantity; the relative motion of the two bodies is, therefore, affected the same way in both cases, and the gravity of the moon to the earth is in both cases lessened.

The action of the moon on the waters of the ocean must be regulated by the same principle. In the hemisphere where the moon is, the waters are more drawn to the moon than the earth is; and its gravity being lessened, the column towards the middle of the hemisphere lengthens in consequence of the pressure of the waters, which are at a distance from the middle point of which the weight is less diminished, or at the horizon even increased. In the opposite hemisphere the earth is more drawn to the moon than the waters, and their relative tendencies are changed in the same direction, and nearly by the same quantity. The attraction of the sun and moon conspire to elevate the waters of the ocean, whether these luminaries be in opposition or conjunction. In both cases the solar and lunar tides are added together, and the tide actually observed is the sum of both. At the quadratures these two tides are opposed to one another, the high water of the lunar tide coinciding with the low water of the solar tide, and vice versa; so that the tide actually observed is the difference between the two.

Newton had no data for measuring the lunar force in producing the tides. But a measure for the solar force, as it acts on the moon, had been obtained. It had been shown to be $\frac{1}{178}$th of the force which retains the moon in her orbit. This last is $\frac{1}{3600}$th of the force of gravity at the earth's surface; and, therefore, the force with which the sun disturbs the moon's motion is $\frac{1}{178} \times \frac{1}{3600}$ of gravity at the earth's surface. This is the solar disturbing force on the moon, distant sixty semi-diameters from the earth's centre; but on a body only one semi-diameter distant from that centre (that is, on the water of the ocean) the disturbing force will be sixty times, less, or $\frac{1}{38448000}$ only of gravity at the earth's surface.

Now this being the mean force of the sun, is that by which it acts on the waters ninety degrees distant from the point where it is vertical, where it is added to gravity, and tends to increase the weight and lower the level of the waters. At the point where the sun is vertical the force to raise the water is about double this; and therefore, the whole force tending to raise the level of the high above that of the low water, is three times the preceding, or about $\frac{1}{12816000}$ of gravity. Newton had previously shown that the centrifugal force, amounting to $\frac{1}{289}$ of gravity, was able to raise the level of the ocean above seventeen miles. This enabled him to conclude, that the elevation of the waters, produced by the solar force, will come out 1·92 French feet. From a comparison of the spring and neap tides, that is, of the sum and difference of the lunar and solar forces, it appears that the force of the moon is to that of the sun as 4·48 to 1. Hence the moon will raise the waters 8·63 French feet, making together 10½ feet, which agrees pretty well with what is observed in the open sea at a distance from the land.

From the force which the moon exerts on the waters of the ocean, Newton concluded that the quantity of matter in the moon is to that in the earth as 1 to 39·78, or, in round numbers, as 1 to 40. He found also the density of the earth as 11 to 9.

Much has been done upon the tides by Maclaurin, Bernouilli, Euler, and Laplace; but the original deduction of Newton, of which an idea has just been given, will be forever memorable.

The motion of comets still remained to be discussed. They had only lately been placed beyond the range of the earth's atmosphere; but with respect to their motion, astronomers were not agreed. Kepler thought that they moved in straight lines, Cassini that they moved in the planes of great circles, but with little curvature. Hevelius had shown the curvature of their path to be greatest when nearest the sun. A parabola having its vertex in that point, seemed to him to be the line in which the comets moved. Newton, satisfied of the universality of gravitation, had no doubt that the orbits of the planets were conic sections, having the sun in one foci. The curve might be an ellipse, a parabola, or an hyperbola, according to the relation between the force of projection and the force tending to the centre. As the eccentricity of the orbit is very great, the portion of it that fell within our view could not differ much from a parabola. This rendered the calculation of the comet's place, when the position of the orbit was once ascertained, more easy than in the case of the planets. From three observations of the comet, the position of the orbit could be determined, though the geometrical problem was one of great difficulty. Newton gave a solution of it, and it was by this that his theory was to be brought to the test of experiment. If the orbit thus calculated was not the true one, the places of the comet calculated on the supposition that it was, and that it described equal areas in equal times about the sun, could not agree with the places actually observed. Newton showed, by the example of the remarkable comet then (1680) visible, that this agreement was as great as could be expected. Thus another proof was given in support of the principle of universal gravitation.

We have been thus particular in tracing the discoveries of Newton, because they constitute the most memorable, the most successful, the most difficult, and the most sublime set of investigations which had hitherto been attempted.—The more the doctrine of universal gravitation has been investigated, the more firmly has its truth been established. Every improvement in the infinitesimal calculus has given mathematicians (if the expression may be permitted) a firm-

er grasp of the universe. New efforts of the mutual action of the planets on each other have been detected; but all according most harmoniously, or rather resulting as a necessary consequence of the law of universal gravitation, as laid down and investigated by Newton. No other department of science can be compared to this; no other branch of human knowledge can be specified, which is built on a foundation so firm that every succeeding investigation has served only to render it more secure. No other theory can be exhibited so perfect, that every minute fact might be deduced *a priori* as a consequence of it; and which does not contain a single phenomenon within the whole range of the science that is not merely not inconsistant with it, but which does not directly flow from it.

The *Principia* appeared in 1687; and the doctrines which they contained were immediately embraced by a small number of British mathematicians who were able to read and understand that immortal work. But on the continent it was treated at first with neglect, and an indifference bordering on contempt. The only mathematical competitor that Newton had on the continent was Leibnitz, with his disciples and staunch adherents, the two Bernoullis. The question respecting the original discoverer of the fluctionary calculus was not yet agitated. Yet the German mathematicians do not seem to have given themselves the trouble of making themselves masters of the Principia. The cautious mode of investigation which Newton had adopted did not quite accord with the genius of Leibnitz, who was fond of metaphysics, and in the habit of introducing them into most of his investigations.

France, which has since that period produced so many eminent mathematicians, owing to the fostering care of her government, could not at that time boast of any of very great eminence.—The philosophy of Descartes was every where prevalent; and his vortices, which it was the object of the Principia to overturn and subvert, were too dear to the French to enable them to judge of the doctrines of Newton with the requisite impartiality. Accordingly, the first mathematician who ventured publicly to defend the doctrines of of Newton was Maupertuis. In the year 1732 he published a work, in which he drew a comparison beween the systems of Descartes and Newton, and showed the superiority of the latter.—Fontenelle, however, in his Eloge of Newton, inserted in the Memoires of the Academy for 1727, admits the infinite merit of the Principia and states the universal shout of admiration, which, after a long interval of years, was raised from every country in his praise. But nothing conformable to the Newtonian principle had at that period appeared in France; we suspect, indeed, that we might say that nothing conformable to it hadappeared in Europe.

It is curious, and contributes not a little to lead us into the character of our lively neighbors, that the great apostle of Newton in France, the person who contributed most to make his opinions known and adopted, was Voltaire.

Fontenelle continued a Cartesan to the end of his days. Cassini and Miraldi seem to have been quite unacquainted with the Newtonian system, and continued the most vague and imaginary hypotheses for calculating the paths of comets, after Halley had computed tables from which the motions of all the comets, that had ever appeared, or ever could appear, might be easily deduced.

We must now proceed to notice the most important additions which have been made in astronomy since the appearance of the Principia.

1. *The abberration of the Fixed Stars.*—Drs Bradley and Molineux, in the end of theyear 1725, were occupied in searching for the parallax of the fixed stars by means of a zenith sector, which was erected at Kew. It was of great diameter, and furnished with a telescope twenty four feet long, with which they proposed to observe the transits of stars near the zenith, according to a method first suggested by Hooke, and pursued by him so far as to induce him to think that he had actually discovered the parallax of γ Draconis, the bright star in the head of the dragon, on which he made his observations. They began to observe the transits of the same star on the 3d of December, when the distance from the zenith at which it passed was carefully marked. By the observations of the following days, the star seemed to be moving south; and about the beginning of March in the following year, it had got 20″ to the south and was then nearly stationary. In the beginning of June it had come back to the same situation where it was first observed; and from thence it continued its motion northwards till September, when it was about 20″ north from where it was first seen; its whole declination amounting to 40″.

This motion surprised the observers a good deal; as it lay the contrary way to what it would have done had it proceeded from the parallax of the star. But the repetition of their observations proved their accuracy. They were afterwards continued by Bradley, with another sector of smaller radius, but large enough for the purpose; which embraced a larger arch, and admitted the observation of stars which passed at a greater distance from the zenith. Even with this addition, the observation did not put Bradley in possession of the complete fact. They only gave him the motion of each star in declination, but not in right ascension.

After considering the subject with the greatest attention, and excluding all those causes which were inadequate to produce the effect, it occurred to him that the apparent motions might be produced from the progressive motion of light, combined with the motion of the earth in its orbit.—If the earth were at rest, it is evident that a telescope, to admit a ray of light from a star to pass along its axis, must be directed to the star itself. But if the earth (and of course the telescope) be in motion, it must be inclined forward, so as to be in the diagonal of a parallelogram, the sides of which represent the motion of the earth and the motion of light, or in the direction of these motions, and in the ratio of their velocities. It is with the telescope just as with the vane at the mast-head of a ship. When the ship is at anchor the vane takes exactly the direction of the wind; but when the ship is under way it places itself in the diagonal of a parallelogram, of which one side represents the velocity of the ship, and the other the velocity of the wind.

The telescope, therefore, through which a star is viewed, and by the axis of which the position of the star is determined, must make an angle with the straight line drawn to the star; except when the earth moves directly to the star, or directly from it. Hence it follows, that if the star be in the pole of the ecliptic, the telescope must be pointed forwards in the directioon of the earth's motion, always by the same angle; so that the star would be seen out of its true place by that angle, and would appear to describe a circle round the pole of the ecliptic, the radius of which subtended at the earth an angle, of which the sine is to unity as the velocity of the earth to the velocity of light.

These velocities Bradley took at 1 to 10313, as most suitable to his observations, which made the radius of the circle of aberration 20″, and the transverse axis of the ellipse, or the whole change of place, 40″. It was the shorter axis which Bradley had actually observed in the case of γ Draconis, that star being very near the solstitial colure, so that its changes of declination and of latitude are almost the same.

To show the truth of his theory, he computed the abberration of different stars, and, on comparing the results with observation, the coincidence appeared almost perfect, so that no doubt remained concerning the truth of the principle on which he had founded his calculations. He did not explain the rules themselves. Clairaut published the first investigation of them in the Memoirs of the Academy of Sciences for 1737. Simpson also gave a demonstration of them in his *Essays*, published in 1740.

2. *Figure of the earth.* Newton, from the theory of universal gravitation, had shown that the figure of the earth was an oblate spheroid, flattened at the poles. But the trigonometrical measurement of France, begun by Picard in 1675, and finished by Cassini in 1716, led to a different conclusion. For the degrees on the north of Paris, instead of lengthening, as ought to have been the case, according to Newton's theory, were shorter by about $\frac{1}{430}$th part than those to the south of that capital. This indicated the diameter of the earth through the poles longer than the equatorial diameter, in the proportion of 96 to 95. This was laid hold of by the Cartesians in their reasonings against the Newtonian philosophy.

A clock made by Graham, having been carried out to Jamaica in 1732, by Colin Campbell, was ascertained by astronomical observations at the Black River, in latitude 18°, (allowing for the influence of heat on the pendulum) to go 1′ 58″ every day slower than in London. Bradley found that this variation exceeded what would suit the figure calculated by Newton. He, therefore, suspected some diminution of gravity in the equatorial regions. But the subsequent investigations of Maclaurin demonstrated the accuracy of the Newtonian conclusions. The French triangulation being resumed in 1740, an error was detected in the measurement of the base, and another not less considerable in the observation of the meridional arc. But the work was not terminated till the year 1754. In the extent of above eight degrees, their lengths appeared regularly to increase from Perpignan to Dunkirk by about $\frac{1}{1730}$th part, marking obviously the oblateness of the spheroid.

Condamine, in the year 1733, urged the Academy of Sciences to send a party to measure a degree under the equator. The academy adopted the project with zeal, and were lucky enough to obtain the concurrence and support of government. In May, 1735, the academicians, Condamine, Bouguer, and Godin sailed from Rochelle to Peru, where they were joined by Juan and Ulloa, two naval officers, deputed by the King of Spain. In the month of July, 1736, they met at Quito, under the line. They chose a valley of the Cordilleras, running about 200 miles southwards from that city, and enclosed on both sides by the loftiest ranges of the Andes. Divided into two sets, they carried a series of triangles along the flanks and summits of those mountains, and connected them with the base measured below. The task was peculiarly arduous, owing to the severity of the climate, and the total want of accommodation. But eight years of indefatigable industry enabled them to finish the measurement assigned to them. Bouguer, the most eminent of them all, gave a complete narrative of all their various operations in his *Treatise on the Figure of the Earth*, not published till 1749; and one of the most scientific works that has ever appeared. He concluded that the earth is not only an oblate spheroid, but so considerably flattened as to have its equatorial diameter to its axis as 179 to 178.

Meanwhile Maupertuis prevailed on the French minister, Maurepas, to despatch another company, which he reluctantly consented to lead, for a similar purpose, to the Arctic Circle. The associates of Maupertuis were Monnier, Camus, Orthier, and Clairaut, by far the most eminent of them all. They arrived in Stockholm, in June, 1736, and were joined by Celsius, the professor of astronomy at Upsala, who had brought from London Graham's zenith sector and transit instruments. They proceeded to the bottom of the Gulf of Bothnia, and selected Torneo as their principal station. Their triangles extended from this town to Kittis, a distance of about sixty miles. The whole was finished in little more than a year; and the length of a degree of latitude at the Arctic Circle was found to be 57,419 toises, or 349 toises longer than the corresponding measurement at Paris. This gave the ratio of 178 to 179 to the polar and equatorial diameters —very nearly agreeing with the more extensive operations afterwards performed in Peru.

But this arctic triangulation, bearing evident marks of haste, became suspected of inaccuracy; and at the suggestion of Melanderhielm, the Swedish academy, about the beginning of the present century, sent Svanberg, with proper assistants, to resume the operations. They not only rectified the former observations, but carried the measurement about forty miles farther north. It was determined that the length of a degree in Lapland is only 57,209 toises. This, compared with Cassini's measurement in France, reduces the oblateness to $\frac{1}{290}$th of the axis. Comparing the measurements in Lapland and Peru, we obtain the ratio of 302 to 301 for the equatorial and polar diameters.

Other measurements were made at the Cape o

Good Hope, in the papal territories, in England, in the East Indies, but as they have not served to diminish, but rather to increase the anomalies, we need not notice them here. The convertible pendulum of Captain Kater, which he was very sanguine would furnish an invariable standard of lineal measure, has been also employed as a means of deducing the figure of the earth. But the results vary not less than those obtained by trigonometrical measurement. Mr. Ivory, from a critical examination of all the data, has concluded the ellipticity to be $\frac{1}{300}$. In this decision, from the consummate mathematical skill of the author of it, we are disposed to coincide.

3. *Application of mathematics to calculate all the disturbances introduced by gravitation into the solar system.*

The first person who improved the Newtonian theory of the moon was Calandrini, professor of mathematics in Geneva, who superintended the printing of the Jesuits' copy of the Principia in 1739 and 1742. He investigated, by a direct method, the principal lunar equations, and likewise the smaller inequalities which Newton had left undemonstrated. He revised the investigation of the motion of the apsides; but his calculations only gave half the quantity derived from observation. Dr. Matthew Stewart, professor of mathematics in Edinburgh, discovered the true motion of the line of the apsides by a simple geometrical procedure. And in 1749, Walmesley produced a correct analytical investigation of the motion of the lunar apogee, which he extended, and completed in 1758.

Clairaut began his investigations of the lunar theory in 1743. At first he was satisfied with merely studying the Newtonian procedure, and converting it into analytical expressions; but he gradually pushed his investigations farther, and in 1747 comprised all the subordinate motions of the moon under the famous general problem of the *three bodies*. But after prodigious labor, his solution assigned for the variation of the lunar apogee only half the measure established by observation. Euler and D'Alembert arrived at a similar conclusion about the same time. Clairaut resumed the subject, and, after incredible labor, obtained a result which accorded perfectly with observation, and thus confirmed the simple law of gravity, as laid down by Newton. The knowledge of this result induced Euler to resume his investigations, and by quite a different procedure he also obtained the true variation of the lunar apogee. D'Alembert pushed his calculations still farther, and approached still nearer the result of observation. Thus the law of attraction was for ever established on the secure basis of mathematical demonstration.

This great point being settled, mathematicians set themselves with eagerness to improve the lunar tables, which were obviously of such importance for finding the longitude at sea. Clairaut bestowed intense application on the subject, and produced a set of lunar tables, distinguished by their superior accuracy. Euler devoted to the same task the whole of his unrivalled analytical skill. But Mayer was the astronomer who distinguished himself most in this important investigation. He had been appointed director of the observatory of Gottingen in 1751, and labored with so much intensity that he shortened his days. He derived the elements of his lunar tables from a discussion of numerous eclipses and occultations. He borrowed little from theory, though he preferred the arrangement of the elements adopted by Euler. He was the first person that employed conditional equations to find the true value of the co-efficients. His tables were inserted in the Grottingen transactions; and after the most careful corrections, he sent them in 1755 to London for the patronage of the Board of Longitude. At his death in 1762, he left two copies, greatly improved, one of which his widow transmited to that scientific body. After protracted deliberations, the sum of £3000 was at last awarded to his family, with a present of £300 to Euler for his excellent formulas. But another more complete copy having been afterwards presented, the Board of Longitude bestowed an additional reward of £2000 at the instance of Dr. Maskelyne, who zealously undertook the editing of those tables in the year 1770.

The next point to which mathematicians directed their attention, was the investigation of the disturbing influence or mutual perturbations of the larger and nearer planets. Euler, in 1747 sent to the Academy of Science a most ingenious memoir on the derangement of Saturn's motion, occasioned by the superior attraction of Jupiter. It was now that he discovered that there exist really no secular equations, but that all deviations from the regular course are strictly periodical, and return always in the same order, though separated at vast intervals. His first investigation was rather imperfect; but four years after he produced another dissertation, which gained the double prize of the Academy. He found that the mean motions of Jupiter and Saturn are equally subject to a very slow increase or diminution, which alternates, however, in the lapse of 15,000 years. He gained the prizes for 1754 and 1756, by his theory of the inequalities in the earth's motion caused by the planets. He discovered four small anomalies to result from their combined attractions, though it was scarcely possible, for want of proper data, to assign the precise measures of these aberrations. He estimated the mean progression of the aphelia at 12″ annually, and the diminution of the obliquity of the ecliptic at 49″ in a century. He found that the eccentricities of the aphelia of Jupiter and Saturn are periodical, and complete their cycle in the space of 30,000 years.

The same subject was discussed by Clairaut in 1757. By comparing his formula with the observations of Lacaille, he determined very nearly the masses of the principal planets, and showed that the greatest effect of their accumulated influence in deranging the earth's motion can amount only to about a minute. His estimate of the attraction of Venus has been confirmed by later and more elaborate calculation.

In 1749 D'Alembert investigated rigorously the effects arising from the moon's attracting the spheroidal prominence of the earth. By the transformation of this general expession he found the *precession* or conical motion of the terrestrial axis about the poles of the elciptic to be 50″ annually, and the *nutation*, or alternate vibration on the same plane to be only 18″ during the period

of the revolution of the lunar nodes. Comparing this quantity with observation, he concluded, that at the surface of the earth the attraction of the sun is to that of the moon as 3 to 7; which makes the satellite to have only the 70th part of the mass of our planet.

Astronomers now turned their attention to the motions of the comets. These bodies describe elliptic orbits with very different inclinations, and so extremely elongated as to resemble parabolas, through a considerable part of their course. Being very small, they are seen for a short space only in the vicinity of the sun, and become quite invisible when they approach the other extremity of their orbit, which is probably beyond the boundaries of our planetary system. The periodic time of comets depending upon the transverse axis of the ellipsis can seldom be determined with accuracy. The few observations which can be made while it is near the perihelium are scarcely sufficient to assign its mean motion, and the places of its nodes and perihelium.

Halley applied the formulas of Newton to the computation of twenty four remarkable comets. But his attention was particularly fixed on the nearest of them, which had been observed in 1531, 1607, and 1682, and seemed to be the same with one noticed in old chronicles in 1080, 1155, 1230, 1305, 1381, and 1456. Hence it performed its revolutions in about 75½ years. He therefore ventured to predict its return about the end of 1758 or beginning of 1759. The time of the expected return approaching excited intense curiosity in the scientific world. Clairaut applied his formulas to the investigation of the progress of this comet. He found that the last revolution would be retarded 618 days longer than the preceding, from the attractions of Jupiter and Saturn. He fixed the time of its appearance to the 4th April. This exceeded that of observation by twelve days only. The discrepancy was probably owing to the influence of Uranus which, was not yet discovered. The comet was first seen by a peasant in Saxony on Christmas day; but soon became the admiration of Paris, and procured for Clairaut the enthusiasm of popular applause.

Clairaut, eager to complete a work in which he had gathered so many laurels, proceeded to calculate the disturbing influence of Jupiter and Saturn on the place of the nodes of the comet of 1682 and 1759, which has an inverted motion. Newton had shown that the perturbations in the planetary system always advance the perihelium and retract the nodes. But the case here was just reversed, and the quantity of recession thence determined agreed most exactly with observation.

But the comets in traversing our system often suffer such derangements that the most select observations are insufficient to determine, with any sort of precision, their elliptical orbits. The famous comet of 1759 was calculated by Euler and Lexell to perform its revolution in a period between 449 and 519 years, while Pingré assigned it a period of 1231 years. In some cases the observations have indicated an hyperbolic orbit. Conti, following the method of Gauss, found the comet of 1811 to revolve in 3056 3 years; but by a second computation, he reduced the time to 2301 years. Bessel gave the comet of 1807 a period of 1953.2 years, but afterward brought it down to 1483.3 years.

Though the comets suffer such great derangement from the large planets, they have no sensible effect on our system. They must therefore be exceedingly small. They consist of a dark nucleus, invested with a cloudy or hazy excrescence, and usually provided with a very long tail. They have never disturbed our tides, though having sometimes approached within the third part of the distance of the moon, they would, with the same mass, exert twenty-seven times greater deranging force. But their passage was so rapid that time was not given to produce the requisite effect on the ocean.

In 1764, Lagrange, who, at the early age of 23, had invented the *calculus of variations*, gained the prize offered by the Academy of Sciences, for his memoir on the *libration* of the moon. He explained in the most satisfactory manner, from the theory of attraction, the cause why the moon always presents nearly the same face towards the earth. He again resumed the subject in 1780.

The theory of Jupiter's satellites is of great importance for finding the longitude. In 1766, Lagrange embraced the subject in its fullest extent, by introducing into his equations, not only the attractive force of the sun but the mutual attractions of the satellites themselves. His investigation was a model of analytical research; yet it did not descend into all the practical details.

In 1773, Laplace having found that the variation of eccentricity of Jupiter's orbit must cause a corresponding alteration in the motion of the satellites, transferred the same idea to the perturbations of the moon, and thus discovered the true theory of the secular equation, or rather of that vast cycle in which the lunar revolutions are alternately accelerated and retarded. During this discussion he demonstrated that the attractive force of gravity must be transmitted 50 million times faster than light, which travels at the rate of 200,000 miles in a second. We may therefore consider it as quite instantaneous. This conclusion is important, because it sets aside all mechanical attempts to explain gravitation by the interposition of an ether, &c, and demonstrates it to be a principle ordained by the wisdom of the Supreme Architect. Laplace continuing his researches, at last discovered that the secular equation of the moon affecting her mean motion, and that of her perigee and her nodes, in the ratio of 4, 12, and 3, is produced by the slow variation of her solar attraction, occasioned by the changing eccentricity of the earth's orbit, resulting from the influence of the larger planets, though they cannot alter the great axis which determines the mean periodic revolution.

In 1785 he proved that Jupiter and Saturn can have no secular equations. But remarking that their mean periods are commensurable, and very nearly as 2 to 5, he found their reciprocal acceleration and retardation to follow the same ratio.—The cycle began in 1560, and comprehends 929 years. So that, in 1750, Saturn had his period shortened 48′ 44″; while that of Jupiter was lengthened by 19′ 28″. In 1788 he discovered two curious laws that connect the periods of Jupiter's satellites, and gave a complete theory of

their motions, which served as the basis of Delambre's excellent tables. In 1808, Lagrange gave a general solution of the problem of disturbing forces, and reduced his equations into a form of the utmost simplicity and elegance.

4. *Lunar and solar parallaxes.* The nearest celestial bodies are seen from the surface of the earth in a position somewhat different than if viewed from the centre. This difference, called *parallax*, is obviously greatest at the horizon, and diminishes constantly as we approach the zenith. To ascertain parallax, therefore, with tolerable precision, observations must be made at distant stations. Lacaille selected the Cape of Good Hope, where he determined the mean parallax of the moon to be 57′ 39″. The parallax of the sun being very small, is more difficult to determine. Kepler had made it a minute, Halley estimated it at 25″; but succeeding astronomers had reduced it to 10″ Halley proposed a very ingenious method of determining it with accuracy from the next transition of Venus, by measuring the acceleration of the time of her passage over the disc of the sun, as viewed from remote points on the surface of the globe. Aware that his own life would not be prolonged till that event took place, he warmly exhorted his successors to prepare themselves for observing it on the 5th of June, 1761. Astronomers were accordingly despatched by the maritime powers of Europe to all the stations that were considered as most accessible and eligible; but the result did not answer their expectations; some of the stations were not well chosen, some of the most expert astronomers had not reached the station assigned them, while others were prevented from observing by the state of the weather. Pingré deducted a parallax of 10½″, while Short made it only 8½″. The uncertainty was finally removed by the numerous and skilful observations of the transit of the 3d of June, 1769. The several results differed scarcely a quarter of a second, and their concurrence fixed the parallax 8″.6. This agrees with the theoretic calculations of Laplace, from the lunar anomalies. Bessel, with immense labor, combined and recomputed the original observations, and detected a small inaccuracy, the correcting of which reduces the parallax to 8″.575. Consequently the mean distance of the sun is 95,158 English miles.

5. *Discovery of Uranus.* Dr. Herschell, who has devoted himself to the construction of telescopes, and to an indefatigable observation of the heavens, announced to Dr. Maskelyne, that, on the night of the 13th of March, 1781, he observed a shifting star, which from its smallness he judged to be a comet, though it was distinguished neither by a nebulosity nor by a tail. The motion of the star was so slow as to require distant observations to ascertain its path. It was for several months presumed to be a comet; but the hypothesis of a parabolic orbit led to very discordant results. The president Saron was the first who conceived it to be a planet, having inferred from the observations communicated to him that it described a circle with a radius about twelve times the mean distance of the earth from the sun. Lexell, before the end of the year, had computed the elements of the new planet with considerable accuracy, making the great axis of its orbit nineteen times greater than that of the earth, and the period of its revolution eighty-four years. Bradley, mistaking it fo ra fixed star, had observed it on the 3d of December, 1753, and it was again seen by Mayer on the 23d of September, 1756.

Herschell gave this new planet the name of the Georgium sidus; but the term *Uranus*, applied to it by Bode, is almost universally adopted. Herschell discovered the satellites of this planet, which are six in number. They revolve in a plane nearly perpendicular to the orbit of the planet, and contrary to the order of the signs. Both these primary and secondary bodies obey, in their revolutions, the laws of Kepler. The same remark applies to the satellites of Saturn, which Herschell increased to seven.

6. *Discovery of four new planets.* Kepler was of opinion that the harmony of our system wanted a planet between Mars and Jupiter. A similar notion was entertained by Lambert, who thought it likely that such a planet might exist, though it was dark and invisible. The distances of the planets from the sun may be denoted by the series 4, 7, 10, 16, 28, 52, 100; which numbers are convertible into English miles by multiplying by 9½ millions. The dark or deficient planet corresponds to twenty-eight, or a distance of 266 millions of miles from the sun.

In the place assigned to this dark planet, four very small bodies have been discovered; which some persons have conjectured to be only fragments dissevered from the principal, and that other similar fragments may still remain undiscovered. The discovery of these very small planets distinguished the commencement of the present century. Piozzi discovered Ceres at Palermo, on the 1st of January, 1801; Olbers discovered Pallas at Bremen, on the 28th of March, 1802; and Harding observed Juno on the 2d of September, 1804, and Vesta on the 29th of March, 1807.—These small bodies differ from the other planets, not only by their diminutive size, but by the remarkable inclination of their orbits to the plain of the ecliptic; which, however, they intersect nearly in the same nodes.

7. *Extent of the Universe.* Herschell, by means of his excellent reflecting telescope, employed himself assiduously in observing the numerous clusters of nebulosities, and distinguished many of the changing and double stars; which, though suns of other systems, yet appear connected, and may, probably, circulate about their common centre of gravity. The subject has lately been carried a great deal further by Sir James South and Sir John Herschell, the latter of whom is preparing to go to the Cape of Good Hope, to examine the double srars of the southern hemisphere.

Sir William Herschel assuming that the instrument which he had used could enable bim to penetrate 407 times farther than Sirius, he reckened 116,000 stars to pass in a quarter of an hour over the field of view, which subtended an angle of only 15′. If wc compute from such a narrow zone, the whole celestial vault must display, within the range of telescopic vision, the stupendous number of more than five billions of stars. If each of those be a sun to a system similar to ours, and if the same number of planets revolve around it. then the whole planets in the universe will be more than fifty-five billions, not reckoning the satellites, which may be much more numerous

What an unimportant portion of the universe is occupied by our earth, and how insignificant must its inhabitants, and all their cares, and pride, and vanity, appear to that Almighty being whose providence regulates and governs at once such a prodigious number of worlds, with all their innumerable inhabitants.

OPTICS.

THE history of astronomy, the most perfect and sublimest of all the sciences, has unavoidably extended itself to a considerable length. Optics is the next science in point of perfection. Like astronomy, it depends on observation, but is every where capable of the rigid application of mathematical calculation.

As light propagates itself in straight lines, it seems to offer itself spontaneously to the consideration of geometers. Euclid accordingly began the investigation at least fifty years before Archimedes had placed mechanics among the number of the mathematical sciences. Euclid only established two particulars: 1. That a point in any object is seen in the direction of a straight line from the eye to that object. 2. That when a point in an object is seen by reflection from a polished surface, the lines drawn from the eye and from the object to the point whence the reflection is made, are equally inclined to the reflecting surface.—From a treatise on optics by Ptolemy, discovered some years ago in the king of France's library, it appears that many observations had been made on refraction, though the law followed had not been discovered.

About 1000 years after Ptolemy, or in the 11th century, Alhazen wrote his treatise on optics. In it he solved a problem of very great difficulty, to find the point in a spherical speculum, at which a ray coming from one given point shall be reflected to another given point. Alhazen was acquainted with the anatomy of the eye, though he did not fully understand the uses of the different parts.—On seeing single with two eyes, he made the very important remark, that when corresponding parts of the retina are affected we perceive but one image. Roger Bacon, who lived in the 13th century, made a near approach to the knowledge of lenses, and their use in assisting vision. The date of the discovery of spectacles, for assisting the sight, may be carried back as far as 1313, but no farther. The inventor appears to have been a Florentine, called *Salvino del' Armato*. He was of a Patrician family. Maria Manni quotes the following inscription on his tomb, from on antiquarian author: *Qui diace (giace) Salvino d' Armato degl' Aumati, Firenze invento de egl' Occhiali, anno* MCCCXVII.

Two centuries later Maurolicus appeared. He was acquainted with the crystalline lens of the eye, and formed a correct judgement of the defects of *short-sighted* and *long-sighted eyes*.—Babtisto Porta, a Neapolitan, invented the camera obscura about the year 1560. The light was admitted through a small hole in the window shutter of a dark room, and gave an inverted picture of the objects from which it proceeded on the opposite wall. A lens was not employed in the first construction of this apparatus, but afterwards used; and Porta went so far as to consider how the effect might be produced without inversion.

Kepler was the first person who explained the mechanical action of the eye in vision. He perceived the exact resemblance of this organ to the *camera obscura*, the rays entering the pupil being collected by the crystalline lens and the other humors of the eye into *foci* which paint on the *retina* the inverted images of external objects. The mind perceives these images and refers them at the same time to things without.

Antonio à Dominis, Archbishop of Spalato, was the first person who explained the production of the rainbow. Having placed a bottle of water opposite to the sun, and a little above his eye, he saw a beam of light issue from the underside of the bottle, which acquired different colors in the same order and with the same brilliancy as the rainbow, when the bottle was a little raised or depressed. From comparing all the circumstances, he perceived that the rays had entered the bottle, and that after two refractions from the convex part, and a reflection from the concave, they returned to the eye tinged with different colors, according to the angle at which the ray had entered. The book containing this discovery was published in 1611.

The telescope was invented about this time, but somewhat earlier. The honor of the discovery belongs to Middleburg, in Zealand. Two different workmen, Zachariah Jans and John Lapprey have each testimonies in their favor, between which it is difficult to decide. The former goes back to 1590, the latter comes down to 1610. Zachariah Jans was an optician in Middleburg, and Boreel has published a letter from his son assigning 1590 as the date of the invention, and another from his sister assigning 1610 as the date. From the account of Boreel it would appear that Jans was the real inventor, and that the discovery of the telescope had been preceded by that of the microscope. News of this discovery was communicated to Galileo in 1610. He immediately constructed a telescope, turned it to the heavens, and made the interesting discoveries which have been already particularized.

The theory of the telescope required that the law of refraction should be discovered. This discovery was the work of Snellius, a mathematician of the Low Countries. To express this law he supposed a perpendicular to the refracting surface, at the point where the refraction is made, and also another line parallel to this perpendicular at any given distance from it. The refracted rays as it proceeds, will meet this parallel, and the incident ray is supposed to be produced till it do so likewise. Now, the general truth which Snellius found to hold, whatever was the position of the incident ray, is, that the segments of the refracted ray and of the incident ray, intercepted by these parallels, had always the same ratio to each other.

In the triangle formed by the two segments of the rays, and by the parallel which they intersect, the said segments have the same ratio with the sines of the opposite angles; that is with the sines of the angles of incidence and refraction. The law, therefore, comes to this, that in the refraction of light by the same mediums, the sine of the angle of incidence has to the sine of the

angle of refraction always the same ratio. It was first stated in this way by Descartes in his *Dioptrics*, in 1637. But Snellius's law, which comes to the same thing, had been publicly taught in his lectures by professor Hortensius, and must therefore have been known to Descartes.

Descartes next entered upon an inquiry, in which he was successful. In ordinary cases of refraction, by spherical and other surfaces, the rays are not collected into one point, but have their foci spread over a certain surface, the sections of which are the curves called caustic curves. The focus of opticians is only a point in this surface, where the rays are more condensed, and of course the illumination more intense than in other parts of it. But if refraction is to be employed either to produce heat or light, it would be a great advantage to have all the rays which come from the same point of an object united accurately after refraction, in the same point of the image. This led Descartes to inquire into the figure which the superficies separating two transparent media of different refracting powers must have, that all the rays diverging from a given point, might by refraction at the said superficies be made to converge to another given point. He showed that curves proper for generating such superficies by revolution have always two given points from which, if straight lines be drawn to any point in the curve, the one of these, plus or minus, that which has a given ratio to the other, is equal to a given line. When the given ratio is one of equality, the curve is a conic section, and the two points its foci. The curves in general are of the 4th or 2d order, and have been distinguished by the name of the ovals of Descartes.

But no practical advantage has resulted from this investigation. The mechanical difficulties of working a surface into any figure but a spherical one, are so great that they have never been overcome.

The next improver of optics was James Gregory, whose *Optica Promota* appeared in 1663. It contained an account of his reflecting telescope. The imperfection of the images formed by spherical lenses led him to substitute reflection for refraction in the construction of the telescope. Gregory thought it necessary that the specula should have a parabolic figure; and the execution proved so difficult, that the instrument, during his lifetime, was never brought to any perfection. After his death, the specula were constructed of the ordinary spherical form, and the Gregorian telescope, till the time of Herschell, was more in use than the Newtonian. It is curious, that though the optics of Descartes had been published twenty-five years, Gregory was ignorant of the law of refraction, and found it out by his own unassisted exertions.

Barrow's lectures on optics, delivered at Cambridge in 1668, treated of all the more difficult questions which had occurred in the then state of the science, with the acuteness and depth which are found in all his writings.

About this time the Jesuit Grimaldi made known some optical phenomena, which had hitherto escaped detection. Having stretched a hair across a sun-beam, admitted through a hole in the window shutter of a dark chamber, he was surprised to find the shadow much larger than the natural divergence of the rays could have led him to expect. Other facts of the same kind made known the general law of the *diffraction* or *inflexion* of light, and showed that the rays are acted on by bodies, and turned out of their rectilinear course, even when not in contact, but at a measurable distance from the surfaces or edges of such bodies. Grimaldi gave an account of these facts in a treatise printed at Bologna in 1665.

The *dioptrics* of Huygens, though a posthumous work, was the production of his early youth, and is said to have been a favorite book with Newton himself. It contains a full development of every thing relating to the construction of telescopes, particularly in that which concerns the indistinctness arising from the imperfect foci into which rays are united by spherical lenses. Rules are deduced for constructing telescopes, which, though of different sizes, shall have the same degree of distinctness, &c. Huygens was a practical optician; he constructed telescopes with his own hands, and some of his object glasses were of the enormous focal distance of 130 feet.

His theory of light was communicated to the Academy of Sciences of Paris in 1678, and afterwards published with enlargements in 1690.—Light, according to him, consists of certain undulations, communicated by luminous bodies to the etherial fluid which fills all space. This fluid is composed of the most subtile matter, is highly elastic, and the undulations are propagated through it with great velocity in spherical superficies, proceeding from a centre. The ingenuity of the theory appears most conspicuous in the explanation which it affords of the constant ratio which subsists between the sines of the angles of incidence and refraction in the same medium. But the happiest application of all, is to the explanation of the double refraction of Iceland crystal, or transparent calcareous spar. This crystal has not only the property of refracting light in the common manner of glass, water, and other transparent bodies; but it has also another power of refraction, by which even the rays falling perpendicularly on the surface of the crystal are turned out of their course, so that a double image is formed of all objects seen through these crystals. This property is not confined to calcareous spar; but belongs, in a greater or less degree, to all substances which are both crystalized and transparent.

The common refraction is explained by Huygens, on the supposition that the undulations in the luminous fluid are propagated in the form of spherical waves. The double refraction is explained on the supposition that the undulations of light, in passing through the calcareous spar, assume a spheroidal form; and this hypothesis, though it does apply with the same simplicity as the former, yet admits of such precision, that a proportion of the axes of the spheroids may be assigned, which will account for the precise quantity of the extraordinary refraction, and for all the phenomena dependent on it, which Huygens had studied with great care, and had reduced to a small number of general facts.

To render the theory of Huygens quite satisfactory, a reason ought to be assigned why the undulations of the luminous fluid are spheroidal in the case of crystals, and spherical in all other

cases. This would render the generalization complete. But till a connection is clearly established between the structure of crystallized bodies, and the property of double refraction, the theory must remain imperfect.

We now come to the discoveries of Newton, which constitute nearly as important an era in optical sciences as his theory of universal gravitation did in astronomy. His researches began in 1666, when he was only twenty-three years of age.

In hopes of improving the telescope, by giving to the glasses a figure different from the spherical, he had procured a glass prism, in order, as he tells us, to try with it the *celebrated phenomena of colors*. These trials led to the discovery of the different refrangibility of the rays of light, now too well known to require to be particularized.

Having admitted a ray of light into a dark chamber through a hole in the window-shutter, and made it fall on a glass prism, so placed as to cast it on the opposite wall, he was delighted to observe the brilliant coloring of the sun's image, and not less surprised to observe its figure, which, instead of being circular, was oblong in the direction perpendicular to the edges of the prism, so as to have the shape of a parallelogram, rounded at the two ends, and nearly five times as long as it was broad.

When he reflected on these appearances, he saw nothing that could explain the elongation of the image, but the supposition, that some of the rays of light, in passing through the prism, were more refracted than others; so that the rays which were parallel before they fell on the prism, being some of them more refracted than others, diverged from one another after refraction, the rays that differed in refrangibility differing also in color. The *spectrum* would thus consist of a series of circular images, partly covering one another, and partly projecting one beyond the other, from the *red*, or least refrangible ray, in succession to the *orange*, *yellow*, *blue*, *indigo*, and *violet*, the most refrangible of all.

But he did not adopt this as the true explanation, till he had tried every other hypothesis by the test of experiment, and proved its fallacy. Even after these rejections, his explanation had still to abide the sentence of an *experimentum crucis*.

Having admitted the light, and applied the prism as before, he received the colored spectrum on a board about twelve feet distant from the prism, and pierced with a small hole. The colored light, which passed through this second hole, was made to fall on a prism, and afterwards received on the opposite wall. It was then found, that the rays which had been most refracted by the first prism were also most refracted by the second, though no new colors were produced. It was also observed, that when the rays which fell on the second prism were all of the same color, the image formed by refraction was truly circular, and of the same color with the incident light.

When the sun's light is thus admitted, first through one aperture, and then through another at some distance from the first, and is afterwards made to fall on a prism, as the rays come only from a part of the sun's disc, the spectrum has nearly the same length as before; but the breadth is greatly diminished; in consequence of which, the light at each point is purer, it is free from penumbra, and the confines of the different colors can be more accurately traced. It was in this way that Newton measured the extent of each color; and taking the mean of a great number of measurements, he assigned the following proportions, dividing the whole length of the spectrum, exclusive of the rounded terminations, into 360 parts. Of these the

Red occupied	45
Orange	27
Yellow	48
Green	60
Blue	60
Indigo	40
Violet	80

Newton conceived that there was an identity between these numbers and the divisions of the monochord, by the notes of music. But it has been since observed, that the spaces occupied by these colors differ according to the kind of prism employed. Hence the relation was merely accidental.

Thus colors were found to be original properties of light, connected with the different degrees of refrangibility belonging to the different rays. Each of these colors, though primary, may be produced by a mixture of the two colors upon either side of it in the prism. Red and yellow make orange; yellow and blue make green; and so on. In general, two colors not very far distant in the natural series, when mixed, make up the intermediate color. All of them together constitute whiteness. Natural bodies of whatever color, if viewed by simple and homogeneous light, are seen of the color of that light, and no other.

Thus supplied with so many new and accurate notions respecting colors, it was natural for Newton to supply them to explain the rainbow.—Every thing respecting that beautiful phenomenon had been already explained, except the colors. As these colors were the same as in the solar spectrum exhibited by the prism, and in the same order, it could hardly be doubted that they were owing to the same cause. He showed this to be the case, by calculating the extent of the arch, the breadth of the colored bow, the position of the secondary bow, its distance from the primary, and by explaining the inversion of the colors.

As all color comes from the rays of light, we must conclude that the color of every particular body is owing to its reflecting the particular kind of ray of which it exhibits the color. If you cast upon a colored body a homogeneous ray of light, it appears much more vivid; that is, it reflects much more light when the ray cast upon it is of its own color. Transparent bodies sometimes reflect one kind of ray, and transmit another.

But here a difficulty occurs. Suppose a body to reflect red or green light, what is it that decomposes the light, and separates the red or the green from the rest? It was this difficulty that led Newton to study the colors produced by light passing through thin plates of any transparent substance. When two transparent bodies, such as pieces of glass having their surfaces somewhat convex, are pressed together, a black spot is formed at the contact of the two, which is surrounded with colored rings, more or less regular,

according to the form of the surfaces. To analyse this phenomenon, Newton made experiments with surfaces of a regular curvature, capable of being measured. He took two object glasses, one a plane convex for a fourteen feet telescope, the other a double convex for one of about fifty feet, and upon this last he laid the other, with its plane side undermost, pressing them gently together. At their contact, in the centre was a pellucid spot, through which the light passed without suffering any reflection. Round this spot was a colored ring, exhibiting blue, white, yellow, and red. This was succeeded by a pellucid or dark ring; then a colored ring of violet, blue, green, yellow, and red, all copious and vivid, except the green. The third colored ring consisted of purple, blue, green, yellow, and red. The fourth consisted of green and red; those that succeeded became gradually more dilute, and ended in whiteness. It was possible to count as far as seven.

The colors of these rings were so marked by peculiarities in shade and vivacity, that Newton considered them as belonging to different orders; so that an eye accustomed to examine them, on any particular color of a natural object being pointed out, would be able to determine to which order in this series it belonged.

Thus we have a system of rings or zones surrounding a dark central spot, and themselves alternately dark and colored; that is, alternately transmitting the light, and reflecting it. It is evident, that the thickness of the plates of air interposed between the glasses, at each of those rings, must be a material element in the arrangement of this system. Newton, therefore, undertook to compute their thickness. Having carefully measured the diameters of the first six colored rings, at the most lucid part of each, he found their squares to be as the progression of the odd numbers, 1, 3, 5, 7, &c. The squares of the distances, from the centre of the dark spot to each of these circumferences, were therefore in the same ratio; and, consequently, the thickness of the plates of air, or the intervals between the glasses, were as the numbers 1, 3, 5, 7, &c. When the diameters of the dark or pellucid rings, which separated the colored rings, were measured, their squares were found to be as the even numbers 0, 2, 4, 6; and, therefore, the thickness of the plates, through which the light was wholly transmitted, were as these numbers.

The curvature of the convex glass being the thickness of the plates of air which corresponded to the different rings, was computed. An inch being divided into 178,000 parts, the thickness of the air for the first series, or for the luminous rings, was $\frac{1}{178000}$ $\frac{3}{178000}$ $\frac{5}{178000}$, &c, inch. For the second series $\frac{2}{178000}$ $\frac{4}{178000}$, &c, inch.

When the rings were examined, by looking through the lenses in the opposite direction the central spot appeared white, and in other rings red was opposite to blue, yellow to violet and green to a compound of red and violet—the colors formed by the transmitted and reflected light being what are now called complimentary, or nearly so, of one another; that is, such as when mixed produce white.

When the fluid between the glasses was different from air, as when it was water, the succession of rings was the same; the only difference was, that the rings themselves were narrower.

When experiments were made on thin plates in such a way that the plate was denser than the surrounding medium, as in the case of soap bubbles, the same phenomena were observed to take place.

To the different degrees of density, then, in transparent bodies, there seemed to be attached the powers of separating particular colors from the mass of light, and of rendering them visible, sometimes by reflection, and in other cases by transmission. Now, as there is reason to think that the atoms of bodies are transparent, they may be conceived to act on light after the manner of thin plates, and to produce, each according to its thickness and density, its appropriate color, which therefore becomes the color of the surface.

But these experiments led to other conclusions, new and unexpected. It was impossible to observe, without wonder, the rings alternately luminous and dark, that were formed between the two plates of glass in the preceding experiments, and determined to be what they were by the different thickness of air between the plates, and having to that thickness the relation already specified. A plate, of which the thickness was equal to a certain quantity multiplied by an odd number, gave always a circle of the one kind; but when the thickness was the same quantity multiplied by an even number, the circle was of another kind; the light, in the first case, being reflected; in the second, transmitted.

Light penetrating a thin transparent plate, of which the thickness was m, $3m$, $5m$, &c, was decomposed and reflected; the same light penetrating the same plate, but of the thickness 0, $2m$, $4m$, &c, was transmitted, though in a certain degree also decomposed. The same light, therefore, was transmitted or reflected, according as the second surface of the plate of air, through which it passed, was distant from the first, by the intervals 0, $2m$, $4m$, or m, $3m$, $5m$; so that it became necessary to suppose the same ray to be successively disposed to be transmitted, and to be reflected at points of space separated from one another by the same interval m. This constitutes what Newton called *Fits of easy transmission and easy reflection.* It constitutes one of the most singular parts of his optical discoveries. It is a necessary inference from the phenomena; and though the cause cannot be assigned, it must, notwithstanding, be admitted as a general fact, which enables us to explain various phenomena that were unknown at the time when Newton lived.

Newton's explanation of refraction proceeds on the theory that light is an emanation of particles moving in straight lines with incredible velocity, and attracted by the particles of transparent bodies. When, therefore, light falls obliquely on the surface of such a body, its motion may be resolved into two; one parallel to that surface, and the other perpendicular to it. Of these the first is not affected by the attraction of the body, which is perpendicular to its own surface; and, therefore, it remains the same in the refracted as it was in the incident ray. But the velocity perpendicular to the surface is increased by the at-

traction of the body; and whatever be the quantity of that velocity, its square, on entering the same transparent body, will always be augmented by the same quantity. But it is easy to demonstrate, that if there be two right angled triangles, with a side in the one equal to a side in the other, the hypothenuse of the first being given, and the squares of their remaining sides differing by a given space, the sines of the angles opposite to the equal sides must have a given ratio to each other. This amounts to the same thing with saying, that, in the case before us, the sine of the angle of incidence is to the sine of the angle of refraction in a given ratio. This explanation of the law of refraction is so satisfactory, that it affords a strong argument in favor of the system which considers light as an emanation of particles from luminous bodies.

Huygens, indeed, deduced from the hypothesis of the vibrations of an elastic fluid, an explanation of refraction which is highly ingenious, but not quite so satisfactory as the Newtonian; though it be the fashion at present to give a preference to the hypothesis of Huygens, we think that very solid objections might be started against it, were this the proper place for such an attempt.

The square that is added to that of the perpendicular velocity of light, in consequence of the attractive force of the transparent substance, is properly the measure of the quantity of that attraction, and is the same with the differences of the squares of the velocity of the incident and the refracted light. This is readily deduced from the ratio of the angle of incidence to that of refraction. When this is done for different substances, it is found that the above measure of the refracting power of different bodies is nearly proportional to their densities, with the exception of those which contain much combustible matter, which is always accompanied by an increase of refracting power.

Thus the refracting power, (ascertained in the way just mentioned) when divided by the density, gives quotients not very different from one another, till we come to combustible bodies, when a great increase immediately takes place. In air, for instance, the quotient is 5208, in rock crystal 5450, in common glass nearly the same; but in spirit of wine, oil and amber, the same quotients are 10121, 12607, 13654. In diamond he found the quotient 14556. Hence he conjectured that the diamond was, at least in part, a combustible body. The refracting power of water being great for its density, its quotient being 7845, he concluded that an inflammable substance enters into its composition—a conjecture which was verified nearly a century afterwards by the synthetical experiments of Mr. Cavendish.

The reflection of light from the surfaces of opaque bodies, and from the anterior surfaces of transparent bodies, appears to be produced by a repulsive force exerted by those surfaces, at a determinate but very small distance; in consequence of which, there is stretched out over them an elastic web, through which the particles of light, notwithstanding their incredible velocity, are not always able to penetrate. In the case of a transparent body, the light which, when it arrives at this outwork is in a *fit* of easy reflection, is reflected back again. The particles on the other hand, which are in a state that disposes them to be transmitted, overcome the repulsive force, and entering the transparent body, are attracted and refracted as already explained. If these rays reach the second surface of the transparent body with a certain obliquity, the attraction will not suffer the rays to emerge into the rarer medium, but will force them to return back into the transparent body. Thus the reflection of light, at the second surface of a transparent body, is produced not by the repulsion of the medium into which it is about to enter, but by the attraction of that which it was going to leave.

In consequence of these discoveries, Newton perceived a defect in the refracting telescope, proceeding from the unequal refrangibility of the different rays. This defect he was of opinion it was impossible to remedy, because he thought that the quantity of the refraction and of the dispersion in different substances bore always the same ratio to each other. Had this been so, the appearance of prismatic colors in a refracting telescope could never have been cured without destroying the refraction altogether. But this was one of the few points about which Newton was mistaken. The mistake was afterwards discovered by Dollond, who found that, by means of crown and flint glass, the dispersion might be counteracted without injuring the refraction.—This great discovery enabled him prodigiously to improve the refracting telescope; and they are now reckoned so much preferable, that they are superseding the reflecting telescope altogether.

Newton turned his attention also to the inflection of light. He found that the ray, in passing the knife edge, had been both attracted and repelled, and that it had been acted on at the distence $\frac{1}{800}$th of an inch from the edge of the knife. The path of the ray, in passing the knife edge, was bent in opposite directions, so as to form a serpentine line, convex and concave, towards the knife, according to the repulsive or attractive forces which acted at different distances. In his opinion, the refraction, reflection, and inflexion of light are all produced by the same force differently modified, and do not arise from the collision of light with the particles of bodies.

The great improver of achromatic telescopes, after Dollond, was Feaunhofer. He observed, in examining the different-colored rays of the spectrum, by means of a telescope, that though the colored spaces appear not parted by any distinct boundaries, yet they are broken and subdivided by numerous white and black lines. He reckoned altogether above 600 lines: a few occur in the red; but they are numerous in the orange, yellow, green, and blue. A stripe, opened by a fine white line, divides the red; other stripes emerge at intervals between the orange and the blue, and two very broad approximating bars cross the violet.—Other glass prisms, and even those filled with liquids, gave similar appearances. It would appear from this that refraction proceeds by irregular bounds, somewhat like Newton's easy fits of transmission and reflection.

We shall finish this hasty sketch of optics, with noticing the discovery of what is called *polarized rays of light*, by Malus, a French officer of engineers. The double refracting property of Iceland spar, which had been so carefully ex-

amined by Huygens, drew also the attention of Newton, who concluded that the ray which suffers the *unusual* or *extraordinary* refraction must have its opposite sides affected by some virtue like magnetism, which gives them a tendency to polarity. This remark lay neglected for almost a century. Malus had early turned his attention to the more difficult problems in optics; and, after his return from foreign service, he resumed his favorite studies. In one of his frequent visits to the observatory, during his residence in Paris in the summer of 1809, he was struck with the brilliant reflection of the setting sun, from one of the windows of the Luxembourg palace. On looking at the appearance through a prism of rock crystal, which he slowly turned round, he remarked, with surprise, that one of the images changed regularly to brightness from obscurity. Next morning he repeated his observation with the same results; and soon found that light, reflected at a certain angle from the surface of glass, acquires the same character as the extraordinary ray in the double refracting prism. Water showed a similar disposition, but at a different angle of incidence.—The law was traced through various reflecting surfaces, and Malus was prosecuting the subject with rapid success, when his career was cut short by a lingering disease, on the 23d of February, 1812. The subject of polarization has been successfully prosecuted in this country by Herschell and Brewster; and on the continent by Biot, Arago, and Fresnel; but much still remains before it reaches the simplicity which characterizes the other branches of optics.

HYDROSTATICS, &c.

The law which determines the weight of bodies immersed in fluids had been discovered by Archimedes, and likewise the position of bodies floating on them. It was ascertained by Stevinus, that the pressure of fluids is proportional to their depth. These two propositions constitute the leading principles of hydrostatics.

Hydraulics, or the motion of fluids, is a much more difficult subject. The first step was made by Torricelli, who proved that water issues from a hole in the side or bottom of a vessel with the velocity which a body would acquire by falling from the level of the surface to the level of the orifice. This proposition, which constitutes the leading principle of hydraulics, was published at the end of Torricelli's *De Motu gravium et projectorum.*

We owe to Torricelli another discovery no less important. The question had been put to Galileo, why water will rise in a pump to the hight of thirty-three feet, but no farther. This question he was unable to answer—though it could not but be obvious from it that the reason assigned by the ancients for the rise of water in a pump, namely, Nature's abhorrence of a vacuum, could not be the true one. Torricelli, reflecting on the subject, conceived that, if a heavier fluid than water were used, a vacuum might be produced in a much shorter tube. He filled a glass tube three feet long, open at one end, and shut at the other with mercury, and covering the open end with his finger, immersed it in a basin of mercury. The result is universallly known. He found the mercury suspended in the tube to the hight of about thirty inches; an effect which he ascribed to the pressure of the atmosphere. It was easy to see that the rise of water in pumps was owing to the same cause.

The objections made to this explanation were removed by carrying a barometer to the top of the Puy de Dome, at the suggestion of Pascal. The mercury fell lower in the tube, because the pressure of the atmosphere was less at the top than at the bottom of the mountain.

The invention of the air-pump by Otto Guericke, burgomaster of Magdeburg, quickly followed that of the barometer. To obtain a space entirely void of air, he filled a barrel with water, and having closed it exactly on all sides, began to draw out the water by a sucking-pump applied to the lower part of the vessel. He had proceeded but a little way, when the air burst into the barrel with a loud noise. After various failures, he at last procured a vacuum by employing a sphere of glass. This was about the year 1654. The air-pump was greatly improved by Mr. Boyle, assisted by Dr. Hooke, who was his operator. For the sphere of Otto Guericke he substituted a more convenient receiver, and made the pump to work more easily, and produced a better vacuum.

Thus the elasticity, as well as the weight of the air, became known. Boyle showed that water always contains air, and he placed the weight and elasticity of air in a variety of new lights.

When we measure the quantity of water discharged from a small hole in the bottom of a cylinder filled with that liquid, the efflux amounts only to about five-eighths of the first emission. To explain this disagreement, Newton had recourse to the supposition of a cataract or funnel-shaped conoid, by which the various streamlets bend their course to the orifice. The theory was first investigated by Daniel Bernoulli, in his treatise on hydrodynamics. It was discussed in 1744 and 1752 by D'Alembert, by Euler in 1755 and 1772, and by Lagrange in 1781. But these admirable analytical investigations have been found of very little utility in the practice of the science. The science is nearly in the state in which it was left by Newton; or at least for the progress which it has made, it is rather indebted to the experimental investigations of practical men than to the abstract reasoning of mathematicians.

The principles of hydrostatics constitute the grounds of naval architecture, which embraces the theory of the construction and sailing of ships. Euler published an important work on the subject in 1749. Bouguer, who, from his situation, possessed much more experience, and was not deficient in mathematical skill, examined the subject in detail, and reduced the theory to a great deal of simplicity. In 1746, he demonstrated a very useful theorem. When a body floats on the surface of the water, it is held in equilibrium by the action of two opposite and equal perpendicular forces, passing through its centre of gravity and its centre of *buoyancy* or that of the fluid displaced by its immersion. Its weight draws it downwards, while its buoyancy presses it directly upwards in the line of support; the vertical position of those centres being essential to floating, the centre of buoyancy does not

remain stationary, but shifts with the inclination of the floating body. The sustaining force may be conceived to act where the line of support crosses the axis. On this point Bouguer bestowed the name of *metacentre*. It is the limiting position, upon which the stability of floating depends. Whenever it stands above the centre of gravity, there is stability; but not so when it is under that centre.

The most complete set of experiments on the resistance of water in narrow canals was made by Bossut between the years 1766 and 1775. He found that the celerity of water was reduced to one-tenth part by flowing through a smooth leaden pipe, whose length is four thousand times its diameter—or that every particle of the fluid has its motion extinguished and renewed ten times during its passage along the pipe. If in this case the whole pressure were divided into a hundred equal parts, one of those only generates the velocity, while ninety-nine parts are expended in merely surmounting the impediments to the flow.

Smeaton's experiments, by means of small models, in 1759, on the action of water and wind against the sails of mills, though made upon a very contracted scale, have been made generally to regulate practice. The experiments of Colonel Beaufoy were made with great care. But the most complete set are said to be those made at Fahlun between 1811 and 1815, by Lagerhjelm and Forselles. Their publication, which appeared in 1818, contains also many experiments on friction.

The resistance of fluids to bodies moving through them is partly occasioned by the accumulation of fluid before them, and partly by the dilatation and consequent partial vacuum produced behind the moving body. Both of these impediments increase at a great rate, according to the velocity. Robins first showed, that when a body moves through air at a rate exceeding 1350 feet per second, a vacuum is formed behind it, and the body in consequence is retarded by a force equal to the whole pressure of the atmosphere. This pressure completely deranges the parabolic theory of the motion of cannon balls. It explains also why the motion of a missile is very much affected by the shape of the posterior extremity.

Robins employed an ingenious method of determining the impulse of balls, by firing them against a heavy-loaded pendulum, whose deflection indicated the momentum thus communicated. By intercepting the flight at different distances, he discovered the diminution of velocity, and consequently the effect produced by the resistance of the air. Dr. Hutton adopted the balistic pendulum in his extensive set of experiments, made on Woolwich Common during the years 1790 and 1791. It comprehended the whole range of velocities from 5 feet in a second to 2,000. The resistance was found to increase in a greater ratio than that of the square of the velocity.

Coulomb showed, by his balance of tortion, that when the motion is very slow, the resistance of fluids is proportional to the velocity simply. In rapid motions it would appear to increase as the square of the velocity. Probably, when the motion is very rapid, the resistance may increase as the cube of the velocity. But this point has not yet been cleared up by a satisfactory series of experiments.

The flow of air through a small orifice suffers nearly the same loss as water, or delivers only about five-eighths of the measure indicated by theory. But when airs pass through a long pipe, the obstruction seems to be considerably greater than happens to water in the same circumstances.

When different gases are made to pass through a small aperture by a given pressure, the time occupied by the various gases is precisely inversely as the square root of the specific gravity of the gases. Thus oxygen gas is 16 times heavier than hydrogen gas; and a given bulk of hydrogen gas will pass through a given aperture, urged by the same pressure, in the fourth part of the time that the same volume of oxygen gas will take to pass.

One of the finest consequences drawn from the principles of pneumatics is the method of determining the hight of mountains by means of barometrical observations. Boyle first observed that the *density of the air is always proportional to the pressure*. This was also announced by Mariotte, in his work on the atmosphere, published in 1676; and, in consequence, is usually known in France by the name of *the law of Mariotte*. In 1685, Halley gave a geometrical demonstration, that the density of the atmosphere diminishes in a continued geometrical proportion, when the hights increase in an arithmetical proportion. For the sake of round numbers, he assumed thirty inches for the standard mercurial column; air at the surface being 800 times less dense than water; and he made the elevation proportional to the difference of the logarithms of these columns. The interval between thirty and twenty-nine inches corresponding to 900 feet. This rule, drawn directly from theoretical considerations, was found to apply, with tolerable accuracy, to an observation made a few years afterwards on the top of Snowdon. Newton generalized the principle in his Principia, by taking into the estimate the decrease of gravity in receding from the earth, and arrived at the conclusion that the densities of the higher strata of the atmosphere form a geometrical progression corresponding to altitudes disposed in harmonic proportion. But this correction, in most cases of barometrical measurement, is superfluous.

After this, the subject fell asleep for almost half a century. Bouguer took the opportunity of his journey to Peru, to compare theory with observation, upon the grandest scale, by measuring the hights of the Cordilleras. His investigation was published in 1753, when he gave the very simple rule which is the ground of our present practice. *That the difference between the logarithms of the barometrical columns, reckoning as integers the first four figures, and deducting a thirtieth part, will express the altitude in toises.* To accommodate the result to English measure, it is only required to add, instead of subtracting the thirtieth part of the logarithmic difference to denote fathoms.

Discrepancies still existed, which were first accounted for by Deluc, who showed that they were owing to the influence of heat on the density of the air. He corrected the defects in the construction of the barometer and thermometer, and determined the amount of the expansion which air undergoes when heated. The rules

which he thence deduced were published in 1772, in his work entitled *Recherches sur les Modifications de l'Atmosphere.* In 1774 they were reduced to English measures, and simplified by Maskelyne and Horsley. General Roy computed and observed the heights of mountains in Wales and Scotland; and Sir George Shuckborough, at the same time, and for the same purpose, ascended the Alps. Their conclusions very nearly agreed, making the difference between the logarithms of the mercurial columns to express the elevation in fathoms at a temperatue only a fraction of a degree below the freezing points.

Very little addition has been made to the data since 1777. A simple proportion given by the late professor Leslie of Edinburgh, however, deserves to be stated, because it is easily remembered, and will enable us to calculate the hights even when we have not in our possession a set of logarithmic tables. It is this: As the sum of the mercurial columns is to their difference, so is the constant number 52,000 to the hight. This number is more easily remembered, from the division of the year into fifty-two weeks.

Something has been done of late years respecting *capillary attraction*, especially by Mr. Ivory, who has unraveled the intricate formula of Laplace, by disjoining it into two separate portions; the one depending on the adhesion of the watery film to the inside of the tube, and the other resulting from half the cohesion of the particles of the liquid to each other. We refer the reader, who wishes for information on this subject, to Mr. Ivory's masterly treatise on the elevation of fluids, published in the supplement to the fourth edition of the Encyclopædia Brittannica.

ELECTRICITY.

This branch of science has been chiefly cultivated by experiment. It is in consequence, full of interest, and exhibits many splendid and striking phenomena. The term is borrowed from the Greek name for amber, and was applied to a property which amber acquired when rubbed. It *attracted light bodies*, and afterwards *repelled* them. To this alternate attraction and repulsion the name *electricity* was given.

The first person who made any striking progress in the investigation of electricity was Stephen Gray, a pensioner of the Carterhouse. His first paper appeared in the Philosophical Transactions for 1720, and he continued to prosecute the subject till the year 1736, when he died. His great discovery was that electricity is capable of passing with great rapidity through metals, animal bodies, green wood, and water; but not through those bodies which may be excited by friction, as glass, hair, silk, feathers, &c. Hence he divided all bodies, as far as electricity is concerned, into *conductors and non-conductors;* the former he could not excite by friction, the latter he could not excite without difficulty. He found likewise, that when a conductor is surrounded by non-conductors, or *insulated*, as it is termed, electricity passes through it to any distance; but when it is in contact with conductors, that electricity is dissipated.

In the year 1733, M. Dufay, whose youth had been spent in the army, turned his attention to electricity, and speedily distinguished himself by three important discoveries. 1. He found that conductors may be excited, as well as non-conductors, by friction, provided we use the precaution to insulate them before we begin to rub. It was from not attending to this necessary precaution, that Mr. Gray had failed in exciting these bodies. 2. Electrical bodies, when excited, *attract* those that are not excited, communicate electricity to them, and repel them. 3. There are two kinds of electricity, which Dufay distinguished by the names of *vitreous* and *resinous.* The terms *positive* and *negative* are now usually applied to them. Bodies having the *same* kind of electricity *repel*, those having *different* electricities *attract* each other. The reason why excited bodies attract light substances to themselves is that they produce in them the contrary electricity from what they themselves possess. Hence they are attracted in consequence of the general law, that bodies having opposite electricities attract each other. Dufay died of the small pox at the early age of forty-one.

In the year 1746, Cuneus, in company with Muschenbrœck and Allamand at Leyden, made an accidental discovery, which drew the attention and astonishment of the whole scientific world. Observing that electrified bodies, when exposed to the common atmosphere, soon lost their electricity, and were capable of retaining but a small quantity, these gentleman imagined that if an excited body were terminated on all sides by electrics, it might be capable of receiving a stronger charge, and of retaining it a longer time. A gun barrel was suspended on silken strings, having one of its ends very near a glass globe, which was turned rapidly, while electricity was excited in it by friction with the hands. A wire was hung from the other end of the gun barrel, which was dipped into a beer glass, full of water, and held in the hands of one of the gentlemen. After the globe had been driven for some time, the gentleman who held the beer glass applied his finger to the gun barrel to draw a spark from it. The consequence was a shock, differing in violence according to the length of time that the globe had been turned. Such was the origin of the famous Leyden phial experiment, which at first excited universal astonishment and terror; but was very soon exhibited in London and Paris, and indeed in all the large cities of Europe, and drew crowds of spectators to witness the phenomena, and to feel the shock.

It was found that this shock could be given at once to any number of persons, provided they laid hold of each other's hands, and that every person (how great soever the number might be) received the shock at the same instant of time. Advantage was taken of this shock to determine the rate at which electricity moves. Dr. Watson made an electric discharge pass through 12,276 feet of wire. One gentleman held a charged Leyden jar in one hand, and the extremity of the wire in the other. Another gentleman held the other extremity of the wire in one hand, and with the other brought a short iron rod in contact with the knob of the Leyden jar. The consequence was a discharge of electricity, and both he gentlemen received the charge at the same

instant of time. So that electricity passes over a circuit of two miles instantaneously, or at least in an interaal of time too short to be noticed. Indeed, in one of Dr. Watson's experiments, the circuit through which the electricity passed was nearly six miles in length, yet no sensible interval of time was taken up by it in this long extent through which it had to pass.

The first successful attempt to explain the phenomena of electricity was made by Dr. Franklin, in a series of letters addressed to Peter Colinson, Esq., from Philadelphia in America. These letters had been offered to the Royal Society, but were refused a place in the transactions of that learned body. When they made their appearance in 1754 they drew universal attention, were almost immediately translated into all the European languages; and if this had not been sufficient, they were ushered into the world in a Latin dress. Dr. Franklin conceived that only one electric fluid existed, and that all the phenomena of electricity were occasioned either by its accumulation in bodies in more than its natural quantity, or by its being withdrawn from bodies, so as to leave in them less than the usual quantity. When a body contains its natnral quantity of electricity, it exhibits no electrical phenomena whatever. When electricity accumulates, the phenomena of the vitreous electricity of Dufay are exhibited. When electricity is deficient, we perceive in it the phenomena of the resinous electricity of Dufay. Besides this theory, which probably occasioned his immediate celebrity, electricity is indebted to him for three capital discoveries. 1. Electricity is dissipated at a great rate by points, so that it is impossible to accumulate it in pointed bodies.

2. The second and greatest discovery of Dr. Franklin was, that lightning and thunder are occasioned by the accumulation of electricity in the atmosphere; or in other words, that lightning is the same thing with electricity. The analogy between lightning and electricity had been pointed out already by the Abbè Nollet, and was generally admitted; but Dr. Franklin having discovered the power of pointed bodies to discharge electricity at a distance, first thought of employing that method to render atmospherical electricity sensible. Not having in America the requisite means for such an experiment, he requested the electricians in Europe to turn their attention to it. The first person who attended to the suggestion was M. D'Abelard, who constructed an apparatus for the purpose at Marly la Ville, about six leagues from Paris. It consisted of an iron rod forty feet long, the lower extremity of which was brought upon a centry-box, where rain could not enter; while, on the outside it was fastened to three wooden posts by silken strings defended from rain. M. D'Abelard was obliged to leave home; but in his absence he entrusted his machine to one Coisier, a joiner, who had served fourteen years among the dragoons, and on whose courage and understanding he could depend. This artisan had all the requisite instructions given him, and he was desired to call some of his neighbors, particularly the curate of the parish, whenever there should be any appearance of a thunder storm. On Wednesday the 10th of May, 1752, between two and three in the afternoon, Coisier heard a pretty loud clap of thunder. Immediately he ran to the machine, taking with him a phial furnished with a brass wire; and presenting the wire to the end of the rod, a small spark issued from it with a snap, like that of a spark from an electrified conductor. Stronger sparks were afterwards drawn in the presence of the curate and a number of other people. The curate's account of them was that they were of a blue colour. an inch and a half in length, and smelled strongly of sulphur. In taking them Coisier received a stroke on his arm, a little below the elbow.

Dr. Franklin, after having published his views on the subject, was waiting for the erection of a spire in Philadelphia to execute his experiment, when it occurred to him that by means of a boy's kite he could have a readier and better access to the region of thunder, than by any spire whatever. Preparing a large silk handkerchief and two cross sticks of sufficient length on which to extend it, he took the opportunity of the first approaching thunder-storm to take a walk into a field where there was a shade convenient for his purpose. He confided his secret to nobody but his son, who assisted in raising the kite.

After the kite was raised, a considerable time elapsed before it exhibited any marks of being electrified. One very promising cloud had passed without effect; when at length, just as he was beginning to despair of his contrivance, the rain having wetted the string, which was of hemp, and rendered it a better conductor, he observed some loose threads on it to stand erect and to avoid one another, just as if they had been suspended on a common conductor. Struck with this promising appearance, he presented his knuckle to the key, which was tied to the end of the string, and the discovery was complete—he perceived a very evident electric spark. Others succeeded even before the string was fully wetted, so as to put the matter past all dispute; and when the rain had wetted the string, he collected sparks in abundance. This happened in June, 1752, about a month after the French electricians had verified the theory, but before any information on the subject had reached Dr. Franklin.

This experiment of Dr. Franklin was attended with extreme danger. Had his cord been more moistened, or a better conductor than it was, it is not unlikely that this celebrated man would have fallen a victim to his temerity. This actually happened soon after to professor Richmann of Petersburg, who was struck dead by a flash of lightning on the 6th of August, 1753, drawn by his apparatus into his room, as he was attending to an experiment which he was making with it.

3. The third discovery of Dr. Franklin was the analysis of the Leyden phial, which contributed more than any thing else to establish his peculiar theory of electricity. The Leyden phial in its present state of simplicity was the work of Watson and Nairn. It consists of a glass jar, of any size, narrower at the mouth, and coated within and without with tin foil to within two or three inches of the mouth. The mouth has a piece of dry varnished wood fitted into it; through which there passes a wire having a knob on the top of it, which rises a few inches above the mouth, and which touches the tin foil coating at the bottom of the jar in the inside. When the knob of this

jar is presented to the prime conductor of an electrical machine, sparks pass into it, and for every spark which passes into the inside of the jar, there may be seen a corresponding spark passing between the outside coating and a metallic knob held near it. The inside of the jar is charged with positive electricity, and the outside with negative electricity. Whenever the inside and outside coating are brought into contact, as by holding the jar in one hand and touching the knob with the other, the two electricities rush into combination and an electric shock is felt.

The Franklinian theory was exhibited in a mathematical dress by Æpinus in 1759, and by Mr. Cavendish in 1771. The theory proceeds on the notion, that only one electrical fluid exists, and that its particles repel each other, but are attracted by matter. But several of the hypotheses which they were obliged to assume are hardly admissible. Thus they were obliged to admit that matter repels matter, though of this repulsion no proof can be given. Neither are the phenomena of electricity consistent with the notion, that an affinity exists between matter and electricity. For it bas been ascertained, that electricity is entirely confined to the surface of bodies, and never penetrates into the interior; a fact which appears inconsistent with any such affinity.

In 1779 a highly important work on electricity was published by Lord Mahon, afterwards Earl of Stanhope. In this work the author enters at large into the doctrine of *electric atmospheres*. He showed, as had been previously done by Mr. Canton, that the air in the neighborhood of a charged conductor gradually acquires the same kind of electricity with the conductor, and that the intensity of this state varies inversely as the square of the distance from the conductor.

In the same work he explained the nature of what he called the *returning stroke*. When an excited body is brought into the neighborhood of another body, the equilibrium of the electricity in that body is disturbed. Let the first body be charged with positive electricity; the extremity of the second body next it will become *negative*, in consequence of the positive electricity of the first body attracting the negative of the second and repelling the positive, which last will accumulate in the farthest extremity of the second body. When matters are in this state, let the surplus electricity be discharged from the first body by touching it with a conductor. The positive electricity which had accumulated at the farthest extremity of the second body will now return and spread itself equally over the whole body. The same will happen to the negative electricity, which had accumulated in the end nearest the first body. It is to this return or new disbribution of the electricity that Lord Stanhope gave the name of *returning stroke*. Suppose a large cloud charged with positive electricity, and that one end approaches within a certain distance of the earth. That part of the earth nearest it will become negative. The negative electricity will accumulate, and the positive will be repelled to a distance. Let the electricity be suddenly discharged from the cloud, the positive electriclty will return back to the part of the earth below the cloud with impetuosity, while the negative electricity will recede with equal velocity. Hence it may happen that animals may be destroyed by such a returning stroke, even when no thunder is near them. A remarkable instance of this took place at Coldstream in the year 1786, when a man and two horses were killed by the returning stroke, soon after passing the Tweed.

Coulomb appeared as an electrician about the year 1785, and added more important facts than any who had appeared after the time of Franklin. He began by constructing a very delicate electrical balance, which measured the attractive and repulsive forces by the tortion of a very fine wire. The three discoveries which he made, by means of this balance, constitute, in fact, the basis of all electrical theory. These discoveries are the following:

1. The attractions and repulsions of electrical bodies vary inversely as the square of their distances.

2. When insulated bodies are charged with electricity, they gradually lose that charge. This is partly owing to the surrounding atmosphere, which never being absolutely free from conducting particles, these particles gradually get possession of the electricity, and carry it off. It is partly owing also to the electric bodies, which serve as insulators. There is, probably, no substance absolutely impervious to electricity; though some are more and some less so. Hence the electricity gradually passes off along the insulated body. Coulomb determined the effect of both these causes, and thus put it in the power of electricians to calculate how much electricity an insulated body loses in a given time.

3. When electricity is accumulated in any body, the whole of it is deposited on the surface, and none of it penetrates into the interior. So that a hollow sphere, how thin soever its walls may be, is capable of receiving as great an electric charge as a solid body of the same dimensions.

From the various experiments of Davy in 1822, there is reason to conclude that the electric spark is merely evolved by the sudden condensation of air, or any elastic fluid through which the electricity passes. For though electricity passes readily through an absolute vacuum, no spark whatever can be perceived.

Electricity may be evolved in some bodies by heat as well as friction. This is is the case with the tourmalin, the electrical properties of which were investigated by Bergman and Æpinus.—When a transparent tourmalin is heated at 212°, one of its ends becomes positive, and the other negative, while the centre of the prism is a neutral point, exhibiting no signs of electricity whatever. From 212° the electric energy increases rapidly till the temperature sinks to 156°, when it is a maximum. From 156° to 104° it remains nearly stationary, and sinks rapidly from 104° to 70°, when it is nearly the same as at 212°. If we break the tourmalin in two while in a state of excitement, each of the fragments will be found to possess precisely the electrical properties of the whole crystal.

The tourmalin is not the only mineral which exhibits these properties. Four or five other crystallized minerals possess similar properties.

Becquerel has lately shown, that when two

elastic bodies are pressed strongly against each other, provided both be insulated, they acquire different states of electricity, the one becoming positive and the other negative. Iceland spar, sulphate of lime, fluor spar, sulphate of barytes, when pressed against cork become positively electrified, while the cork becomes negative. But cork becomes positive when pressed against cyanite and retinasphaltum. Electricity thus evolved continues a long time.

Pouillet has lately ascertained, that when charcoal is burned, the portion of the piece which is unconsumed becomes charged with negative electricity, while the carbonic acid gas formed is positive. When hydrogen is burned, the air around the flame at some distance is positive, but the centre of the flame is negative.

About the beginning of the present century, a new and most important electric instrument was contrived by Volta, which has been employed with the most fortunate success in promoting the science of chemistry. When two different metals, zinc, for example, and copper, are placed in contact, and then separated, the zinc is electrified positively and the copper negatively. If we take a number of pairs of these two metals, placing each pair in contact and in the same order, and introduce between each pair a card moistened with wet salt-water, a current of electricity is evolved. If we apply the wet finger of one hand to the top of such a pile, and the wet finger of the other hand to the bottom of the pile, we immediately feel an electric shock, the intensity of which increases as the number of pairs of plates. With 100 pairs the shock is severe. Such was the origin of what is called the Galvanic or Voltaic battery. The plates are usually about three or four inches square. Each zinc plate is surrounded with copper, distant from it about a quarter of an inch, except at the top where the two plates are soldered together. These plates are inserted into the cells of a porcelain trough, so fixed that the copper plate of one cell is soldered to the zinc plate of the next cell. The cells are filled with a weak solution of nitric acid. About 200 pairs of plates, thus united, constitute a pretty powerful galvanic battery.

In such a battery, the plate at one end is copper, and that at the other end zinc. The zinc end is positive, the copper end negative. When the two extremities are joined by a conducting wire, a current of electricity passes uninterruptedly through the battery as long as the nitric acid is dissolving the zinc, which it attacks in preference to the copper. But when the acid is saturated with oxide of zinc, the current is at an end. The quantity of electricity thus set in motion is very great, but its intensity is low. When the conducting wires are iron or platinum, they become red hot, and even undergo fusion if they are not too long or too thick. Pieces of charcoal, attached to platinum wires from each extremity of the battery, when placed in contact, become white hot, and burn, if surrounded with air or oxygen gas, with very great splendor.

If a platinum wire from the positive extremity of the battery be plunged into a glass of water, and a platinum wire from the negative extremity be plunged into the same glass, taking care that the two wires do not touch each other, the water immediately begins to undergo decomposition; oxygen gas being evolved from the positive wire, and hydrogen gas from the negative, exactly in the proportions that exist in that liquid. If the positive wire be iron or copper, oxygen gas is not evolved from it in the requisite quantity, but the wires become oxydized. If a salt be dissolved in the water it undergoes decomposition, its acid being attracted towards the positive pole, and its base to the negative pole. All chemical compounds are decomposed by this battery, if sufficiently powerful, and they are decomposed according to this law. Oxygen, chlorine, bromine, iodine, and fluorine, together with all acids, are attracted to the positive pole, while hydrogen, azote, alkalies, earths, and metals, are attracted to the negative pole.

The same progress has not been made in developing the theory of this curious piece of electrical apparatus, as has taken place in investigating the phenomena which it produces. The action of the battery is limited to the time of the chemical action of the nitric acid on the zinc: hence it is obviously connected with this action. No liquid conductor is found to answer, unless it be capable of acting chemically on the zinc. Nitric acid answers better than any other acid, and it dissolves the zinc more rapidly than any other.

MAGNETISM.

The ore of iron, called the *loadstone*, was known to the ancients, and the property which it possesses of attracting iron filings; but they were ignorant of the *directing* power, which has been turned to such important account in modern navigation. But this directing power, there is reason to believe, was known to the Chinese at least 2000 years ago, though they did not turn it to so good account as the inhabitants of Europe. Whether the knowledge of the mariner's compass was brought from the East is not known. But a citizen of Amalphi, in Italy, whose name is said to have been Flavio Gioyo, first constructed the mariner's compass, such as we have it at present, and marked the north end of the needle with a fleur de lis, the armorial bearing of the kingdom of Naples.

The first person who attempted to generalize the magnetic phenomena was Dr. Gilbert of Colchester, who began his career during the reign of Queen Elizabeth. He found that every magnet, whether natural or artificial, has its powers concentrated in two opposite points, called the north and south poles; intermediate between which there is a neutral point: that similar poles repel, while opposite poles attract, with a force (as was afterwards ascertained by Coulomb) varying inversely as the square of the distance. If a long bar of iron be held in a position nearly vertical, its lower extremity is always found to manifest the properties of a north pole. From this fact, Gilbert inferred the magnetism of our globe.

The variation or declination of the needle was discovered by Columbus during his celebrated voyage of discovery; since which time it has been regularly observed. In 1580, the needle in London pointed 11° 15′ east of north. In the year 1657, it pointed due north. Its declination

increased yearly in a westerly direction till 1814, when it reached its greatest distance west, namely, 24° 21′ 10″. Ever since 1814 it has been going east at a very slow rate.

The dip or inclination of the magnetic needle was first observed by Norman, in the year 1576. If a steel needle, suspended freely from its centre of gravity, be converted into a magnet, it will no longer hang perpendicularly; one end of it will incline downwards, while the other will rise above the horizontal position. This is called the dip. Norman found the dip of the north pole at London to be 71° 50′. From that time it continued constantly to increase at a very slow rate till the year 1720, when it seems to have reached its maximum; namely, 75° 10′. Ever since it has been slowly diminishing. In 1821 it was 70° 3′.

From numerous observations made in different parts of the torrid zone, there is reason to believe that the line of the dip or the magnetic equator is distant at a maximum about 12° from the terrestrial equator. It is a great circle of the sphere, and cuts the equator on two places in about 113° west longitude, and 67° east longitude. As we advance north or south from this magnetic equator, the dip slowly increases, the south pole dipping if we go south, and the north pole if we go north.

The first person who attempted to account for the declination of the needle, was Dr. Halley. He considered the globe of the earth as constituting one great magnet, having four poles, two to the north, and two to the south, at considerable but unequal distances from the poles of the earth. Two of these poles he considered as fixed, and two as movable. This subject was taken up by Professor Hanstein of Christiania, who has had the advantage of another century of observations. He has adopted the Halleyan hypothesis, with some modifications. One of the north and one of the south poles is weaker than the other. One of the north poles revolves round the pole of the earth in 1740, the other in 860 years. The stronger north pole is at present situated in north latitude 73° 53′, and west longitude 87°; the weaker in north latitude 85° 12′ and east longitude 140° 6′. The stronger south pole is in south latitude 68° 52′, and east longitude 132° 35′; the weaker in south latitude 78° 16′, and west longitude 135° 59′.

In 1722, Mr. Graham discovered the daily variation of the needle. The subject was prosecuted by Mr. Carters, and Colonel Beaufoy determined the amount of the daily variation for a number of years, with indefatigable accuracy. It was taken up at last by Mr. Barlow and Mr. Christie. They all concur in showing that the daily variation is owing to the action of the Sun on the needle altering the intensity somewhat by the effect of his heat.

Mr. Barlow ascertained that the intensity of the action of iron on a magnetic needle is proportional to its surface, not to its solid contents; so that a hollow sphere of iron, however thin, has as great an effect upon a needle as a solid iron sphere of the same diameter.

It was supposed at first that iron or substances containing iron, were alone susceptible of the magnetic influence; but cobalt and nickel were afterwards found capable of being converted into magnets; though less powerful than those of steel. Coulomb showed that all bodies were to a certain extent susceptible of magnetic influence.

While hot, iron has no effect upon the magnetic needle; but as the iron cools it begins to act, and it acts most powerfully at a blood-red heat. Mr. Barlow and Mr. Charles Bonnycastle observed that between a bright-red and a blood-red heat it acts in the opposite way from what it does when cold. If it attract the north pole of the needle while cold, it will attract the south pole at the temperature above specified.

Mr. Barlow found that if a magnetic needle be placed at a certain distance from a mass of iron, the needle is deflected when the iron is put into rapid motion.

About the year 1824, M. Arago discovered that when plates of copper and other substances are set in rapid volution beneath a magnetized needle, they cause it to deviate from its direction, and finally to drag it along with them. This curious fact was repeated and verified in 1825 by Messrs. Herschell and Babbage. They mounted a powerful compound horse-shoe magnet, capable of lifting 20 lbs., in such a manner as to receive a rapid rotation about its axis of symmetry placed vertically, the line joining the poles being horizontal, and the poles upwards. A circular disk of copper, 6 inches in diameter, and 0.85 inch thick, was suspended centrally over it by a silk thread without tortion, just capable of supporting it. A sheet of paper properly stretched was interposed, and no sooner was the magnet set in rotation, than the copper commenced revolving in the same direction, at first slowly, but with a velocity gradually and steadily accelerating. The motion of the magnet being reversed, the velocity of the copper was gradually destroyed; it rested for an instant, and then began to revolve in the opposite direction.

The rate of rotation was not diminished, by interposing between the copper and the revolving magnet, plates of paper, glass, wood, copper, tin, zinc, lead, bismuth, or antimony. But it was greatly diminished when the substance interposed was iron. One sheet of tin plate reduced the rate of rotation to about one-fifth part, and two sheets nearly destroyed it.

When disks of other metals were substituted for copper, they were found also capable of being made to rotate, though with different degrees of rapidity. The order of the different metals, and the intensity of the force of each, that of copper being reckoned 1, was as follows:—

Copper	1.
Zinc	0.90
Tin	0.47
Lead	0.25
Antimony	0.11
Bismuth	0.01
Wood	0.00

When these disks were cut with a pair of scissors in various radii, stretching from the circumference nearly to the centre, the force was greatly weakened; but it was nearly restored again, by soldering these intervals with any metal whatever.

Silver was found to hold a high rank among the metals, and gold a very low one. Mercury lies

between antimony and bismuth. The only other substance, besides the metals, in which any traces of magnetism have been perceived, is charcoal, in that state in which it is deposited at a red heat from coal gas, lining the retorts, and gradually filling them.

Copper revolving produces no effect upon copper. It was always necessary, in order that motion should be produced, that one of the bodies should be a magnet. It is obvious that magnetism is introduced into the copper and the other metals by induction.

The intensity of the directive force of a magnetic needle may be measured, by causing the needle to vibrate in small arches, and counting the number of vibrations performed in a given time; the intensity of the directing force being inversely, as the square of the number of vibrations.

The effect of an electric current upon a magnetic needle was accidentally discovered by professor Œrsted of Copenhagen, in the year 1819. While a galvanic battery was in action, he placed a needle under the wire which united the two poles of the battery, and observed that it placed itself at right angles to the conducting wire.—When placed above the conducting wire, it also placed itself at right angles to it; but in that situation the needle was reversed, the north pole pointing the contrary way from what it did when the needle was under the wire. If the needle be *above* the conducting wire, the north pole turns to the *left* of the current of positive electricity.—If it be below the conducting wire, the same pole turns to the right. When the needle is placed on the west side of the conducting wire, (supposed to be nearly in the magnetic meridian) and parallel to it, the north pole will be elevated; but when placed on the east side of the conducting wire, the same pole is depressed.

Arago and Davy observed, about the same time, that the conducting wire, while the galvanic battery is in activity, attracts iron filings; but the moment the contact is interrupted, the filings drop off; but bodies not magnetic are not attracted by it.

Arago observed, that if a copper wire be wound spirally round a glass tube, and then made to unite the opposite poles of a galvanic battery, if a steel needle be introduced into the inside of the tube it is instantly converted into a magnet. If the copper wire be twisted round a mass of soft iron, shaped like a horse-shoe magnet, and made to unite the opposite poles of a galvanic battery in action, the soft iron becomes a powerful magnet, and retains its virtue while the battery continues in activity; but if we interrupt the connexion of the copper wire with either of the poles of the battery, the magnetic virtue disappears.

Next to the discoveries of Œrsted, those of Mr. Faraday are the most important. He first observed that the position of the magnetic needle, with respect to the conducting wire, greatly modified the effects produced. He ascertained that the apparent attraction of the needle on one side, and its consequent repulsion on the other, did not occur in all circumstances; but that according as the wire was placed nearer to or farther from the pivot of the needle, attractions or repulsions were produced on the same side of the wire. When the wire is made to approach perpendicularly toward one pole of the needle, the pole will pass off on one side, in that direction which the attraction and repulsion at the extreme point of the pole would give. But if the wire be made continually to approach the centre of motion, by either the one or the other side of the needle, the tendency to move in the other direction diminishes; it then becomes null, and the needle is quite indifferent to the wire; and ultimately the motion is reversed, and the needle endeavors to pass the opposite way.

From these facts, Mr. Faraday concluded that the centre of magnetic action, or the true pole of the needle, is not placed at its extremity; but in its axis, at a little distance from the extremity, and toward the middle, that this point has a tendency to revolve round the wire; or, which is the same thing, the wire round the point. He found, upon trial, that the conducting wire of a battery, if at liberty to move, will revolve round the poles of a magnet, while the battery is in action. This discovery has given occasion to a variety of little toys, in which small batteries are made to revolve round the poles of a magnet or magnets round a wire, uniting the opposite poles of a battery in action.

Mr. Faraday has more lately discovered, that when the two poles of a magnet are joined by a piece of soft iron, when one of the poles is disengaged from the iron a spark is perceived; but this spark does not appear when both poles are disengaged together.

From the phenomena of magnetism just enumerated, there cannot be a doubt that electricity and magnetism depend upon the influence of the same principle. What that principle is, may never be discovered. It has been usual to consider it as a subtile and elastic fluid, which revolves round the surface of an excited body in a spiral direction. Doubtless the magnetic poles of the earth result from electric currents. But we are still in the dark with respect to the reason why only steel, nickel, and cobalt can be converted into magnetic needles, and why the magnetic virtue is so weak in the other metals.

CHEMISTRY.

Chemistry, unlike the other sciences, sprang originally from delusion and superstition, and was, at its commencement, on a level with magic and astrology. After shaking off this unhallowed connection, it offered its services to the physician in the humble rank of an assistant in the preparation of medicines. Its services being accepted, it ventured occasionally to investigate the nature of the medicines employed by medical men, to simplify and improve them, and even to suggest the employment of new remedies, when they appeared from the researches made to be superior to the old. From this humble beginning, it gradually extended its benefits to miners, metallurgists, and artists in general; and examining the properties of all the substances in nature, ameliorated, and improved, and simplified many of the most useful processes.

We have no evidence that the ancients were acquainted with chemistry, in the modern sense of the word; but it was cultivated by the Arabians, after the establishment of the empire of

the caliphs. Geber, who lived in the eighth century, is almost the only chemical writer among the Arabians whose works have become known in Europe. Four tracts of his, known by the quaint titles, "Of the Investigation or Search of Perfection;" "Of the sum of Perfection, or the Perfect Magistery;" "Of the Invention of Verity or Perfection;" "Of Furnaces," &c, have been translated into Latin, and likewise into English.

His object was the preparation of a universal medicine; and during his investigation of it, he states a number of circumstances, which enable us to appreciate the degree of chemical knowledge which he possessed, and which may be summed up under the following heads:

1. He was acquainted with gold, silver, copper, iron, tin, lead, mercury, arsenic. Whether he was acquainted with antimony is not so clear.

2. Potash and soda were known to him, at least in the state of carbonates. But whether he was aware of their different properties does not appear.

3. He was acquainted with sulphuric acid, nitric acid, and, probably, muriatic acid; or at least he prepared an aqua regia, by mixing sal ammoniac with nitric acid, which enabled him to form a liquid capable of dissolving gold.

4. He knew common salt, saltpetre, sal ammoniac, alum, sulphate of iron, borax, corrosive sublimate.

5. He was acquainted with red oxide of mercury, oxide of copper, red lead and litharge.

6. He employed vessels for distillation, and used aludels of stoneware, glass, and metal.—The water bath, the common still, the method of distilling per descensum, furnaces for melting and calcining metals were familiar to him. Vinegar was used as a solvent by him in many of his processes.

As chemistry is entirely an experimental science, and as the number of facts and compounds gradually discovered is exceedingly great, it would be impossible, in a sketch of this kind, to notice every individual improvement or discovery. We shall, therefore, satisfy ourselves with pointing out those which had the greatest influence upon the progress of the science.

When we have a compound of two bodies united together, and wish to separate one of them, the successful process is to add a third body.—This third body, in many cases, unites with one of the ingredients of the compound, and disengages the other. Thus saltpetre is a compound of nitric acid and potash, in the proportion of 6.75 of the former, and six of the latter. If we want to procure the nitric acid, we shall succeed if we put 12.75 of nitre into a retort, and pour over it 12.25 of the sulphuric acid of commerce. Upon applying heat to this mixture, the nitric acid of the saltpetre will come over into the receiver, together with the water of the sulphuric acid, making together nine; and there will remain in the retort all the potash, combined with all the sulphuric acid, weighing together sixteen. Magor, in the 14th section of his treatise *De Spiritu nitro-aereo*, published in 1678, seems to have been the first person who explained in an intelligible manner, these mutual unions and decompositions which take place in salts when a ird body is judiciously added. The theory was afterward suggested by Sir Isaac Newton at the end of his Optics. The union he ascribed to an attraction existing between the atoms of bodies, and the decomposition to the addition of a body having a greater attraction for one of the constituents of the compound than the constituent has which it displaces. Tables exhibiting the order in which these decompositions take place, were first drawn up by Geoffroy Junior, in 1718. These tables were gradually improved, till at last they were brought to the greatest degree of perfection which they have yet attained, by Bergmann, in the year 1783.

Sir Humphry Davy made a most important improvement in this theory of attraction and decomposition, and first rendered the way in which decomposition takes place intelligible. The atoms of bodies, according to him, are always in an electrical state; some of them being positive and others negative. Bodies in the same state have no affinity for each other, and, of course, do not unite, unless one of them be much more powerfully positive or negative than the other. When this happens, and the two atoms are brought near each other, the opposite state may be produced in the other by induction, in which case union may take place. Thus oxygen and chlorine are both negative; but oxygen much more so than chlorine. Hence when they approach near each other, the negative state of the former will increase, while that of the latter will diminish, so that it may become actually positive, though only weakly so. Hence they will unite; but the union will not be strong.

Bodies in different states have an affinity for each other, and this affinity augments in proportion to the intensity of the two states of electricity in the two bodies. Thus oxygen is intensely negative, and hydrogen is intensely positive.—Hence there is a strong affinity between them. Sulphuric acid is negative, and magnesia positive; hence they unite, and constitute sulphate of magnesia. Potash is more strongly positive than magnesia. Hence when it is mixed with sulphate of magnesia, it will not only attract the sulphuric acid more strongly than the magnesia does: but it will, at the same time, repel the magnesia, because both are in the same electrical state. This greater attraction, combined with the repulsion, is capable of driving off the magnesia, and enabling the potash to take its place.

The first attempt to generalize the phenomena of chemistry was made by Beccher, and by his successor Stahl. Combustion was the most extraordinary phenomenon in chemistry. It was confined to a certain set of bodies called *combustibles*. When these bodies were heated in the open air, they became hotter of themselves, and continued for a certain time to give out heat and light in great abundance. When the process is at an end, these bodies have lost their original appearance, and have assumed another quite different. Thus when sulphur is burnt, it is converted into an invisible gaseous body, having a strong afid suffocating smell, and a sour taste, and known by the name of sulphurous acid.—Phosphorus is changed into a white solid sour tasted body called phosphoric acid; iron into a *red* powder, lead into a red or yellow powder, and mercury into a black or red powder. Beccher

and Stahl thought that all combustible bodies owe their combustibility to the presence of a peculiar body, which was the same in all, and to which they gave the name of *phlogiston*, (or *the combustible body.*) During the burning of the combustible body the phlogiston makes its escape, and what remains is the other constituent; for all combustibles, in their opinion, consisted of two constituents, phlogiston and an incombustible substance which varied for each body. For sulphur, it was sulphurous acid; for phosphorus, phosphoric acid. For iron, lead, and mercury, the black, yellow, or red powders which remain when these metals are calcined. These opinions were universally adopted by chemists, and modified their views, and regulated their mode of experimenting. Boyle, indeed, had shown that tin, when calcined, increases in weight; but this did not alter their opinions, because the increase of weight was ascribed to *fire* fixed in the tin during the process of calcination.

Chemistry continued in this country little else than the art of preparing medicines, till Dr. Cullen began his great career about the year 1740. He was aware of the important purposes to which the science might be applied, and had devoted himself to the subject. In 1746 he was appointed lecturar on chemistry in the college of Glasgow; and though his attention was speedily withdrawn from his favorite science, by his being placed in the medical chair of that university, yet he had the merit of rendering chemistry popular, and of educating Dr. Black, who became his favorite pupil and friend.

Black, when a very young man, discovered that limestone is a salt composed of two different constituents, namely, lime, which possesses the characters of an alkali or base; and carbonic acid, which possesses the characters of an acid. This last ingredient is a gas or aerial body when in a separate state. Hence it is disengaged when lime is dissolved in an acid, and occasions the *effervescence* which always accompanies such solutions. When the limestone is exposed to a strong heat, the carbonic acid is, in like manner, driven off, and leaves the lime in a separate state. What is called quicklime, or burnt lime, is the pure substance, or caustic lime. When combined with carbonic acid it loses its causticity, and is converted into a mild tasteless salt.

Mr. Cavendish took up the subject of carbonic acid where Dr. Black had laid it down. He determined its specific gravity, its solubility in water, and its various other properties. Bergmann dwelt upon its acid characters, and described the salts which it forms, with the various bases. Dr. Priestley made experiments upon its antiseptic properties, and, together with Dr. McBryde, recommended water impregnated with it as a valuable medicine. Lavoisier determined its nature, showed that it was formed during the combustion of charcoal, and that it was a compound of three parts by weight of charcoal, and eight parts of oxygen. Hence the reason of its formation during the combustion of wood and coal, and all those bodies which contain charcoal.

Mr. Cavendish examined the properties of hydrogen gas, showed how to prepare it by dissolving iron or zinc in dilute sulphuric acid, determined its sensible qualities, ascertained its specific gravity, examined its combustibility, and discovered that, when burnt, it unites with half its volume of oxygen gas, and that the product is water. The subsequent investigations of Dr. Prout and Dr. Thomson have shown that oxygen gas is sixteen times heavier than the same bulk of hydrogen gas. Hence, as water is a compound of one volume of oxygen, and two volumes of hydrogen, it follows that water contains 8-9ths of its weight of oxygen, and 1-9th of hydrogen.

Dr. Priestley discovered oxygen gas on the 4th of August, 1774, by heating the red oxide of mercury in a gun barrel. He found it to be a gas; that bodies burn in it better, and that animals are capable of breathing it longer than common air. He considered common air as a compound of oxygen and phlogiston. When air is saturated with phlogiston, he thought it was converted into azotic gas, which therefore he called *phlogisticated air.* For a similar reason he called oxygen *dephlogisticated air.* But Scheele, who had been engaged in the same investigations, without being aware of the labors of Priestley, took a simpler view of the subject. He considered common air as a mixture of oxygen and azotic gases; both of which he thought were simple bodies.—It was the oxygen that made air capable of supporting combustion, and capable of being breathed. Lavoisier adopted the opinion of Scheele, and betrayed the origin of his views by assuming the very same proportions of oxygen and azotic gas in air that Scheele had done. Cavendish, in 1783, demonstrated that air is a compound of about four volumes azotic and one volume oxygen gas; and he showed, by making trials in the neighborhood of London for a whole year, that these proportions do not sensibly vary. These views, after having been neglected or overlooked by chemists for nearly twenty years, were universally adopted about the beginning of the present century, chiefly in consequence of the observations of Berthollet.

Chlorine was discovered by Scheele in 1774, during his researches on manganese. He found that it was disengaged when muriatic acid was digested on the black oxide of manganese. He observed its smell, its color, and its property of destroying vegetable colors; and he considered it as a muriatic acid, deprived of phlogiston, by which term he understood hydrogen gas. The properties of this gas were farther examined by Berthollet and Chenevix. Berthollet considered it as a compound of muriatic acid and oxygen; and, therefore, gave it the name of oxy-muriatic acid. He had the merit of first proposing it to bleach linen and cotton, a process which was afterward brought to perfection in Glasgow. In 1809, Gay-Lussac and Thenard made a set of experiments, in order to separate oxygen from it; but without being able to obtain any evidence that it contained any. The subject was taken up by Davy in 1810. He was equally unable to obtain any proof of the presence of oxygen in it. He, therefore, affirmed that it was not a compound of muriatic acid and oxygen; but a simple substance, similar in many of its properties to oxygen itself, and, like it, capable of uniting with all the other simple bodies. This induced him to give it the name of *chlorine*, from a Greek word signifying *grass green.* He showed

that when equal volumes of chlorine and hydrogen gases are mixed together, and an electrical spark passed through the mixture, combustion takes place; the two gases unite, and are converted into muriatic acid gas, equal in volume to the bulk of the two gases before combustion.—Hence it follows that muriatic acid is a compound of chlorine and hydrogen. Thus the theory of Scheele was established by Davy.

Bromine was discovered by M. Balard in 1825, and iodine by M. Courtois in 1811. The former is a hyacinth-red liquid, the latter a bluish volatile solid, having the metallic lustre and some resemblance to antimony in its appearance. The properties of bromine were investigated by Balard, and those of iodine by Gay-Lussac and Davy; but chiefly by the former. Both of these bodies are simple substances; both resemble chlorine very closely in their characters, and, like it, they combine with other bodies. The compounds of chlorine, bromine, and iodine, with other substances, are distinguished by the names of *chlorides*, *bromides*, and *iodides;* just as the compounds of oxygen are called *oxides*.

Azotic gas was first noticed by Dr. Rutherford, in his thesis written in 1782. Scheele published his analysis of air, and his investigation of the properties of *azote*, in the year 1777; and Priestley investigated various of its properties. But it is to Mr. Cavendish that the science is chiefly indebted for every thing accurate that has been determined respecting this gas. He ascertained the volume of it that exists in common air, showed that it is capable of combining with oxygen, and that the compound formed is *nitric acid*. It was afterward ascertained that the specific gravity of azotic gas is 0.9722, and that of oxygen gas 1.1111, and that nitric acid is formed by the union of one volume of the former gas, with two and a half volumes of the latter; so that nitric acid is a compound of 1.75 of azote, and five of oxygen by weight.

Dr. Priestly investigated the properties of nitrous gas and protoxide of azote, and pointed out the importance of the former in analysing air.—Davy subjected them to analysis, and obtained pretty near approximations. The subject was afterwards resumed by Dalton, and reduced to its present state of simplicity by Gay-Lussac. These gases are compounds of azote and oxygen; the former of 1.75 azote and two oxygen, and the latter of 1.75 azote and one oxygen.

Before the rise of scientific chemistry in Britain, the two alkalies, potash and soda, were known, but they were not accurately distinguished from each other till Duhamel published his dissertation on the subject in 1737. Lime had been known from the beginning of the world; but its characters were first accurately pointed out by Hoffman. Magnesia had been discovered about the beginning of the 18th century; but chemists were not acquainted with its true character, and were in the habit of confounding it with lime, till its properties, and the salts which it forms, were accurately pointed out by Black in 1765.—Scheele discovered barytes in 1774, and strontain was recognised as a peculiar substance as early as 1789; but its true nature remained uncertain till Dr. Hope published his experiments on it in 1791, and Klaproth his a few months earlier. Lithia was discovered by Arfvedson in the year 1718. These seven bodies were distinguished by the names of alkalies and alkaline earths. Their properties were gradually determined with accuracy, and the striking analogy which they have to each other was recognised; but all the attempts made to determine their constituents were long unsuccessful. At last Davy, in 1808, demonstrated that potash is a compound of oxygen and a white very combustible metal, to which he gave the name of potassium; and soda, of sodium and oxygen. He was able, soon after, to show that lime, barytes, strontain, and magnesia are also compounds of metals and oxygen. And when lithia came to be known, he showed that it also was a compound of the metal lithium and oxygen. Thus it was discovered that all the alkalies and alkaline earths are metallic oxides.

Alumina was known to the alchymists, but its peculiar nature and properties were first determined by Margraaff, in 1754, and afterwards more accurately by Scheele and Bergmann.—*Glucina* was discovered in 1798, by Vauquelin, as a constituent of the beryl and emerald; and yttria, in 1796, by Gadolin of Abo. *Cerium* was made known in 1804, by Klaproth, under the name of *ochroita*, and a little after it was more accurately examined by Berzelius and Hisinger, who gave it the name of *cerium*. *Zirconia* was discovered in the zircon, in 1789, by Klaproth, and afterwards in the hyacinth; and *thorina* was discovered by Berzelius in 1828.

The analogy between these five bodies, and the alkaline earths, was so strong, that as soon as the nature of the latter was ascertained, chemists, in general, inferred that the former also were metallic oxides; but several years elapsed before this opinion was verified by experiment. At last, a few years ago, Œrstedt discovered that if alumina be intimately mixed with dry charcoal, heated in a tube, and a current of dry chlorine gas be made to pass over it, decomposition takes place, and a pale greenish yellow substance sublimes, which is a chloride of aluminum. Wohler found that when this chloride is heated with potassium it undergoes decomposition, the chlorine uniting with the potassium, and the aluminum being set at liberty. The aluminum, or base of alumina, thus obtained, is a white metal in grains, requiring a very high heat to fuse it; probably malleable and permanent in the air, or under water.—When heated in air or oxygen gas it takes fire, burns, and is converted into alumina, by uniting with oxygen.

Glucina was decomposed, and its basis glucinum obtained by the very same process. The same remark applies to yttria. Glucinum and yttrium have a strong resemblance to aluminum. Cerium was first obtained in the metallic state by Laugier, in 1814, and zirconium by Berzelius, in 1824. This last substance wants the metallic lustre, and has a much stronger resemblance to charcoal than to a metal. Thorina was reduced precisely in the same way as alumina. Thorium has also a striking resemblance to aluminum in its properties.

Thus it has been ascertained that all the fixed alkalies, the alkaline earths, and the earths proper, amounting altogether to thirteen, are metallic oxides.

The remaining simple bodies have been divided into two sets; namely, acidifiable bases and alkalifiable bases. The acidifiable bases are eighteen in number. Of these there are two, charcoal and sulphur, which were known to the ancients. All the rest have been discovered by the researches of chemists or chemical physicians. The history of the discovery of hydrogen and azote has been already given.

Borax seems to have been known to Geber in the eighth century. It is found in Thibet and China, and is much used in India as a vehicle for the solution of varnishes, &c. Boracic acid was extracted from it, in 1702, by Homberg, and in 1752 Baron showed that borax is a compound of boracic acid and soda. In 1808, Gay-Lussac and Thenard, by heating boracic acid and potassium, decomposed it, and obtained its basis, to which the name of *boron* has been given. It is a black powder like charcoal, possessing peculiar properties; burning when heated in contact with air or oxygen gas, and being converted into boracic acid.

Silica is one of the most abundant substances in nature, and under the name of *rock crystal* was known to the Greeks. But its chemical properties were first accurately investigated by Bergmann and Scheele. Berzelius, in 1824, succeeded in showing it to be a compound of oxygen, and a black substance called *silicon*, very similar in many of its properties to charcoal, though quite different in others. For example, it does not burn when heated in air or oxygen gas; but it burns when heated with carbonate of potash.

Phosphorus was accidentally discovered by Brandt, in 1669; selenium by Berzelius, in 1818; tellurium was pointed out, with some hesitation, by Muller of Reichenstein, in 1782; and its characters finally determined by Klaproth, in 1798.

The white oxide of arsenic was known to the Arabians; but we do not know who first attended to its metallic basis,—though as this basis is found in the earth in the metallic state, it could not have escaped altogether the attention of miners and metallurgists. Paracelsus seems to allude to it; and Schroeder, in his Pharmacopœia, published in 1649, gives us a process for obtaining it. Its characters were first determined by Brandt, in the year 1733.

The ancients were acquainted with the oxide and sulphuret of antimony, but not with the metal. When the metal became known, we do not know, but Basil Valentine was acquainted with it in the 15th century.

Chromium was discovered by Vauquelin, in 1797; uranium by Klaproth, in 1789; vanadium by Sefstrom, in 1830; molybdenum by Scheele and Hjelm, in 1778 and 1782; tungsten by Scheele and D'Elhuyarts, in 1781; columbium by Hatchett, in 1801; and titanium by Gregor, in 1791.

The alkalifiable bases are thirty-one in number. We have already given the history of the discovery of the first thirteen of these, constituting the bases of the fixed alkalies, alkaline earths and earths proper. The ancients were acquainted with iron, lead, tin, copper, mercury, silver, and gold. Manganese was discovered by Scheele in 1774, and reduced to the metallic state by Gahn soon after; nickel was examined by Cronstedt, in 1751; cobalt by Brandt, in 1733. Zinc appears to have been known in China and the East for many ages; but it was never smelted in Europe till Henkel pointed out a process, in 1721. Cadmium was discovered by Stromeyer, in 1817.—Bismuth occurs native in some of the German mines; and was, of consequence, known as soon as these mines were worked. Agricola mentions it in his Bermannus, published in 1529; but Beccher, Pott, and Geoffroy Junior were the first chemists who investigated its properties.

Platinum was known to the Spaniards soon after the colonization of South America; but its chemical properties were first investigated by Scheffer, in 1752, and still more completely by Lewis, in 1754. Palladium and rhodium were discovered by Dr. Wollaston in 1803 and 1804; and iridium and osmium by Mr. Smithson Tennant about the same time.

The acids at present known to chemists amount to 126. It would be too tedious a task to enumerate them all, and to point out the persons by whom they were discovered. We may, however, state that Scheele did more to improve this important branch of chemistry than any other individual. He made known, and described no fewer than eleven new acids; namely, fluoric, molybdic, tungstic, arsenic, lactic, gallic, tartaric, oxalic, citric, malic, and saclactic. It was he also that first isolated the prussic acid, and commenced that interesting branch of chemistry, which was brought to perfection by Gay-Lussac, when he discovered and determined the nature of cyanogen, and the compounds which it forms.

To Mr. Dalton we are indebted for a branch of chemistry which promises fair to reduce the science to the same accurate state which distinguishes astronomy, by the application of rigid calculation. It had been observed before, that bodies unite in definite proportions. He showed that these proportions might be stated in numbers; and concluded that bodies unite atom to atom, and that these numbers denote the relative proportions of the weights of these atoms. Berzelius first endeavored to obtain accurate atomic numbers by means of analysis. The result has been a great improvement in the accuracy of analysis, and much more correct notions respecting the nature and properties of chemical substances than were formerly entertained. This branch of the science is advancing apace. The number of salts which have been formed, and examined with more or less accuracy, amount to about 2200, and their number is increasing daily.

The application of chemistry to the vegetable and animal kingdoms, the interesting facts laid open respecting vegetable substances and vegetation, and the light thrown upon some of the most important functions of living animals, constitute, if possible, a still more interesting branch of the science than those upon which we have already touched; and that its future progress will develop many new facts still more important than those already known, cannot be doubted. But in a dissertation like the present, which is so much limited in point of extent, it would be impossible to give historical details of even the most important branches of a science, much more extensive than any other to which mankind have devoted their attention, and which is daily extending it-

self still farther, by entering upon important fields of investigation not yet traversed.

Here, then, we shall conclude this historical sketch. We have found it necessary, from the limited space allowed us, to omit much. Yet the view which we have given, imperfect as it is, shows sufficiently the splendid improvements made in the science by the moderns, and the immense blanks still left for posterity to fill up.—Astronomy alone has advanced to a state approaching to perfection; but chemistry, already one of the most important of the sciences, and destined ultimately to become much more so, is still almost in a state of infancy.

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BY DIONYSIUS LARDNER, LL. D.

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LECTURE I.

ELECTRICITY.

LADIES AND GENTLEMEN:—The manner in which I propose to bring to your attention the subject chosen for these Lectures is that which I have uniformly adopted in the whole course of my public teaching. There is nothing which in itself is more interesting than the habit of following the steps by which discoverers themselves have produced their great results; and there is no way in which scientific knowledge may be rendered more interesting than that by which it was originally acquired. I shall, in bringing before you the magnificent discoveries which have been made almost in our own time, follow the course the discoverers themselves have taken. But, in order to conduct this inquiry with profit and interest, it is necessary to put you in possession of the chief natural laws which the investigations of the last hundred years have developed. It may be done in a brief compass, though, truth to say, they were acquired only by a vast, and to us most surprising, deal of labor.

Elaborate and careful investigation has brought to light certain physical facts of an extraordinary nature concerning Electricity. It has been found that all bodies in their natural state—that state in which we commonly see them—are pervaded through their inmost dimensions by two extremely subtle fluids called by the common name of Electricity, but differing in certain respects. Philosophers generally agree to call them *positive* and *negative* electricities. Now these two extremely attenuated fluids, so attenuated that no means have ever yet been able to detect in them the quality of gravitation, pervade all substances. When bodies are in their natural state, these two fluids are contained in equal proportions; any body—a table, chair, &c,—contains an equal quantity of the positive and negative electricities. These two fluids have a property of mutual attraction—each *molecule*, or particle of the one, coalescing with each particle of the other, so as to satisfy their reciprocal forces, the compound being found to produce no effect. A body containing an equal quantity of these is in a state of electrical repose. But if any external cause disturbs this equilibrium, breaks the union of these

two fluids and separates the one from the other, then their mutual attractive force is no longer satisfied, and the fluid becomes an active agent, the body thus disturbed possessing the one and not the other. The body is, in common language, then said to be electrified. I have thus briefly given you the ultimate conclusions which have been reached concerning this agent, and you can put yourselves in the situation of philosophers with regard to it, since you possess all the knowledge concerning it that has yet been unfolded.

There are various ways in which a body may be electrified—in which the natural combination of the fluids may be destroyed. One of the most common is by *attrition*. If two substances, as two leaves of paper, or glass and silk, be rubbed together, the process calls into action a certain cause which decomposes the combination of the two fluids. The positive withdraws from the negative, which is attached to the handkerchief, while the positive adheres to the glass. The disturbance insures a separation, and neither fluid remains in its natural state. Granting this fact, you may ask why is this effect produced? What is there in the character or nature of silk or glass which should cause this result? To this I can only say, that you know as much about it as the most profound philosopher. Nothing more than the naked fact is known; and no principle of science has been discovered which will explain it. The only difference between philosophers and yourselves is, that they may know a greater number of cases: but they have no more general or positive knowledge.

Thus we have as a starting point these facts, that there are two fluids of so attenuated, so volatile, so spiritual a character, that they have no weight, nor can they be perceived in any way but by their effects. It is necessary next to fix in your mind that these two fluids, like light and heat, have a greater facility of moving over certain bodies than over others: for example, the metals transmit heat freely, while glass conducts it not at all. One of the first things to be known is the class of substances most capable of conducting Electricity, and that class which does not do it at all, or at least but imperfectly. The bodies along which it moves with most facility are the metals—all of them, without exception. If you put an electrified body at one end of a metallic rod, the fluid will rush to the other with infinite rapidity, and will be stopped in its career only by meeting some non-conductor. The bodies which refuse to conduct the fluid can hardly be designated by any general appellation; but it may be said that *fibrous* bodies,—such as wool, feathers, &c, are non-conductors. The fibres of wood are bad conductors, while wood itself may be a good one; but in this case the fluid is carried along by the moisture of its pores. Liquids, although conductors, are not very good ones—not equal, by any means, to the metals. To this distinction of bodies into conductors and non-conductors I shall have occasion frequently to refer, and it may be necessary to say that no bodies are absolute and perfect in this respect. The conducting power is to Electricity what transparency is to light. Glass is a conductor of light, although imperfect, while wood is a non-conductor.

I have mentioned, then, two main facts: that all bodies naturally have two electric fluids in a state of combination, and that being electrified consists in disturbing this equilibrium; and that there are two classes of bodies, one capable of conducting this fluid, and the other not. The third point I must notice before proceeding to trace the progress of discoveries in Electricity, is this: I have stated that *attrition* decomposes the combined electricities. The same effect may be produced by a change of temperature, and in various other ways, but by one in particular, which merits notice. If a body in its natural state—say a lump of metal—be brought near an electrified body, the proximity of the latter will decompose the electricities of the former. Suppose the body be electrified positively; the attraction of the positive electricity will draw the negative of the body in its natural state toward the nearest side itself, and repel the positive to the opposite extremity. By this attraction of one and repulsion of the other, the electricities of the bodies are decomposed. This I can only notice briefly now; it will be seen more clearly in unfolding the progress of discovery. These are the main points to be impressed upon your minds at the outset.

The subject of Electricity is one, the merit of whose discovery belongs almost exclusively to modern times. It is a lesson—a moral lesson of great importance, deduced from the progress of knowledge, to observe the manner in which Nature prompts to discovery, and notice the slowness of man in following out her suggestions. There is scarcely a discovery of modern times of which the sure promptings of Nature cannot be traced for many, many an age backward; and we may thus learn how truth, which appears in the end so clear and beautiful, lies for a long while buried and concealed.

At an early period in the history of physical science, the property possessed by *Amber* had been observed. Amber was a substance remarked by the ancients from this circumstance:—that, when rubbed, it had the power of attracting light substances. This mysterious power appeared to them so similar to life, that they were accustomed to say that amber had a soul. This was the extent to which the knowledge of this principle had been carried until an early period in the Christian era, when we find indications of a slight advance. In Cæsar's Commentaries we find mention made of Roman sentinels who had seen, in southern latitudes, flames upon the points of their lances. Whether this is to be regarded as a fact in the history of Electricity may well be doubted. We now know, what was not then suspected, that this phenomenon arose from a principle similar to that observed in amber. Seamen had for a long while been acquainted with the singular appearance of flames upon the tops of masts—known by the names of Castor and Pollux; but they were not possessed of the fact known to us that these were but exhibitions of the electric influence. These facts appear to be but the *cropping out* of Science—forcing her way up through the surface, and intruding her precious ores upon the notice of man. We next find it on record that a certain Gothic chieftain, on drawing off his clothes, perceived his flesh to be on fire. We know now that the attrition of cloths upon natural bodies developes electrical light.

These slight facts may be passed over as mere indications which *might* have afforded traces for discoveries in Electricity, but which were utterly unheeded. It was not till about a hundred years ago that facts so accumulated upon the attention as to acquire a certain general character and lead to the principles of Electricity. The experiments of two individuals in England—GRAY and WHEELER—afforded the first scientific information possessed concerning this mysterious fluid. The first great law which they established was the division of bodies into conductors and non-conductors. Up to their time the main facts known were, that if certain substances be rubbed with cloths they became electrified and would attract feathers, and show other indications of electricity. Gray and Wheeler, in repeating and observing their experiments, happened on one occasion to thrust a piece of cork into the mouth of the glass tube they were using; on applying the light substances, they observed with infinite surprise that they were attracted by the cork which had not been rubbed, as well as by the glass which had. With the spirit of philosophers they determined to carry the matter farther, and ascertain whether a longer cork would exhibit the same property; they tried it, and found that it did. They then attached a deal rod, two or three feet in length, to the cork, and on rubbing the glass, they found that the rod shared in the electrical power. They at once concluded that here was at work some influence before unnoticed, and determined to pursue the experiment until they should reach a more general law.

They procured a hempen cord as a substitute for a deal rod, fastened it to the end of the tube, and suspended at its other extremity an ivory ball. Now, said they, if the fluid flies along the rod, it may along the cord; and, upon holding light substances to the ivory ball, truly enough it attracted them just as the rod had done. This was a most important step. It was clear the fluid passed along the cord, and they resolved to carry their experiment still farther. They accordingly retired to a country-house, owned by Webster, and some miles from London, where they could have more local convenience for their investigations, and proceeded thus: Not finding a sufficient hight from which to suspend their cord, they had recourse to this expedient—which affords another forcible illustration of the important part accident often plays in this discovery of truth.

We shall, indeed, find, if we watch the progress of physical Science, that a large proportion of her immortal principles have owed their discovery solely to accident, and thus shall we find it with this great principle of conduction. Gray and Wheeler suspended their hempen cord; but as they wished to make it longer than any hight they possessed would allow, they carried it along under a long gallery, supporting it by silken threads—which they chose merely because they were fine and strong, and likely to prevent the fluid from escaping by their tenuity. At the other end of the hempen cord they suspended the ball of metal; and on rubbing the glass and presenting light substances to the ball, they found that they jumped up to it, as you all know would be the case. They were, of course, highly pleased with the result, and proceeded to carry the experiment still farther.

They procured a much longer cord; but now their silk cords would not support it, as its weight was too great. 'Let us then,' said Wheeler, 'take fine wire, which will be stronger and finer, and therefore less likely to carry off our fluid than the silk.' Wire they accordingly did take, and on rubbing the glass, and applying light bodies to the ball, found, to their great surprise, that there was not the slightest symptom of attraction. Their first suspicion was that the cord was too long, and accordingly they restored it to its primitive length, still keeping their wires as supporters; but even then no signs of electricity could be elicited. 'What is the reason of this?' said they; 'when we suspend our cord by silk, the experiment succeeds; but when we use wire, we lose all our elestricity. There must be some remarkable difference between these two substances. They followed this point, and saw plainly enough that the electricity escaped along the wire, but would not along the silk. The result of this was the grand discovery—a discovery of the first order in physics, —that the metals are good conductors, while silk, and thence all fibrous bodies, will not transmit the fluid at all, or but imperfectly. This was the first great discovery in electricity, and you see the accident from which it arose.

Soon after the investigations of Gray and Wheeler were undertaken, a French philosopher named DUFAYE had taken up the subject, and pursued it in a spirit different from theirs. They had vast ardor, and multiplied their experiments with a singular zeal; but they had little acuteness, and blundered upon the truths they discovered. Dufaye's mind was of a different order; he made a less number of experiments, but the few he did make led to results of far greater importance. The first was the capital fact that there are two kinds of electricity; this Dufaye clearly showed. If you rub a piece of sealing-wax and hold it to a light body, it will attract it; while glass, being rubbed, repels it. Both were electrified, and Gray and Wheeler supposed that both were electrified in the same way. Dufaye believed that the electricities were possessed of opposite properties. The body attracted by the sealing-wax was repelled by the glass; while the sealing-wax and the glass attracted each other. This was the first grand discovery made by Dufaye.

Gray and Wheeler discovered that some bodies were conductors and others non-conductors. As an immediate consequence of this, an important practical fact ensued. If a body be electrified, there is a continual tendency in the fluid to escape, and it will be entirely lost if any body standing upon the floor be placed in contact with the electrified body. Gray and Wheeler saw that this put into the hands of the electrician a vast and extraordinary power—the power of arresting the fluid—and compelling its presence—of keeping it under his hands until he can submit it to the closest experiments. This is the power of insulation. Suppose a metallic ball be highly charged, and you wish to keep it half an hour, how is it to be done? By the discovery of Gray and Wheeler, it must be suspended by a silk thread or placed upon glass. Dufaye at once seized upon this

and said, 'Now we have the means of arresting this fluid and of compelling it to submit to the severest cross-examination.' Dufaye reasoned farther: we know that if a body be electrified the electricity in escaping will make itself visible by sparks. Now we know that the bodies of animals are capable of being charged with the fluid. If then, said Dufaye, we can manage to charge the person of a man and prevent the fluid from escaping, we may draw a spark from him. With this plan in his head, Dufaye went to the Abbé NOLLET and disclosed his belief that the human body might be charged. Nollet laughed at him, as if the plan could only exist in the head of a crack-brained enthusiast. Dufaye, however, persevered, and they agreed to try the experiment. They went to work clumsily in following out the discovery of Gray and Wheeler. 'Now,' said Dufaye, 'I'll suspend my body by silk cords: it shall then be electrified, and you may try to obtain the spark.' It was agreed, and Dufaye was duly hanged with silk. Electrical machines not having been invented, a glass tube was rubbed, and as fast as electricity was evolved it was communicated to the suspended Dufaye. When it was thought that the philosopher was sufficiently charged, Nollet presented his knuckle to his cheek, and behold! a bright spark passed from one philosopher to the other! This to us is sufficiently amusing, because with us at the present time, it is done every day; then, however, it was unknown, and on being published, all Europe was astonished at the wonderful phenomenon.

The epoch at which the circumstances of electricity were gradually unfolded was from 1730 to 1740. The process of extorting truth from Nature is extremely slow; and in fourteen or fifteen years it is surprising to see how little real truth was developed. The Royal Society of London has always been remarkable for the zeal with which it has engaged in the propagation of scientific knowledge. The Secretary has usually been a man who, without any extraordinary powers of mind, has possessed a strong love of information and an ardent desire for its diffusion. The Secretary, at the time of which I am speaking, was a Mr. Collinson, a name now almost unknown. At this period, also—that is, in 1745—Philadelphia possessed FRANKLIN, then unknown except at home, laboring as a printer in that city. There was then in Philadelphia a Literary and Philosophical Society, either the same as that which now exists, or one out of which it sprung. It has long been the laudable object of the Royal Society of London to call together scientific minds, and to promote communication between scientific bodies all over the world, and thus to aid in the development of truth and of valuable facts in science and the arts. When the circumstances I have recounted attracted general notice, Collinson sent over to the Philosophical Society of Philadelphia one of the glass tubes used in electrical experiments, with written instructions as to the mode of using it. When it arrived, the few whose business allowed them to know or care any thing about the subject, which then had scarcely been heard of in this country, said to Franklin, 'Here is a letter from this gentleman, with a glass tube; look, at your leisure, and see what you can make of it.' Franklin took it in hand in June, 1747; and with that singular ardor of mind, that extraordinary zeal and enthusiasm of character which marked the whole life of Franklin, and which made with him the taking up of any question and the pursuit of it to its extreme limits one and the same thing, within a week he had repeated all the experiments ever made upon the subject, and placed himself in a position to push his inquiries beyond the point the same subject had reached in Europe.

In October he commenced a correspondence with Collinson, and between 1747 and 1752 this was continued on all convenient occasions in a series of letters which, like other scientific writings, were not appreciated at first, but afterwards were highly estimated, and raised Franklin to the highest rank of philosophers. That extreme sagacity that *instinct* (for I can call it nothing else) which enables philosophical minds to go on before experience and in advance of their age, and to behold the 'shadows of coming events,' enabled Franklin to see what distant years alone developed; and with that simplicity of mind, that unbounded ardor and disregard as to who might be deemed the discoverer of any great and important truth, so it were only made known, he communicated to Collinson his belief that *electricity and lightning were the same*, and proposed to draw the fluid from the clouds. The letter was read before the Royal Society at London, and was received by the usually grave philosophers, the members of that body, with shouts of laughter! It was suffered to pass by unnoticed; and no one cared to risk his reputation for sanity by even mentioning this ludicrous fancy-flight, and Franklin was in no danger of being anticipated in his discoveries, or of having its merits claimed by the Royal Society.

Meanwhile Franklin quietly proceeded in his investigations. The first conclusion of his researches arrived at about the year 1748, was the basis of his famous theory of Electricity—a theory which has not stood the test of time, and has indeed almost fallen to the ground, a noble and wonderful wreck. The theory deserves notice not mainly on its own account, but because of the extreme beauty of the reasoning and experiments on which it was based. One of the first experiments was this: Two persons, A and B, stood upon two insulated stools; in the hands of A was placed a glass tube, which was electrified by attrition; B then touched the tube and received a portion of the electricity. Consequently B had an excess of the fluid—more than was natural—while A had less. The process was repeated until B was surcharged; while A lost and B received it from him. This done, a third person presented his knuckle to B and received a spark—from B's surplus—of electricity, as was to be expected. Another effect was exhibited; when the knuckle was presented to A, who lost his electricity, sparks were likewise exhibited. Well, this was explained by Franklin thus: in the first case, when I touch B I receive electricity from his surplus, and hence the spark; in the second, when I touch A, I communicate to him part of my own received from the earth, the common reservoir of electricity.

Upon this was based his theory, which was this: *all bodies are endowed with a certain quantity of electricity; if they have more than their natural quantity they are electrified positively;*

if less negatively. This famous theory of a single fluid, called the Franklinian theory, was almost universally received in England and the United States; but it never obtained general acceptation on the continent of Europe, and the old principle of Dufaye, of two fluids, was still retained in France and the other continental kingdoms. The two theories had their advocates for a long while, and within my own time, public opinion was divided. But when Franklin's theory was brought under the severe scrutiny of mathematics, as it has been within the last 20 years, by processes I need not now explain, it was found that there were circumstances for which it would not suffice; but which could easily be explained by the theory of two fluids, which are often called the *vitreous* and *resinous* electricities. I shall then take it for granted that the mathematical world concur in rejecting Franklin's theory; and the theory prevails that there are two fluids of opposite properties, the natural state of which is in combination.

The theory of Franklin, if it has been rejected, has been made respectable by its beauty and ingenuity; for it is scarcely possible to mention in the history of hypotheses a theory so ingenious and elegant. And still greater will be our admiration when we consider that it was not the product of a scientific mind. Look at the difference between Franklin and Newton; the one was reared in the most profound mathematical university in the world; his associates were mathematicians and philosophers; his emulation was stimulated by their rivalry, and his mind urged to farther inquiry by their daily converse. Surrounded by these advantages, it is impossible to say what lights he derived from their influence. How different was it with Franklin! Reared here, three thousand miles from all scientific pursuits, to matters of business, first as a tallow chandler's apprentice, then as a printer, the wonder is not that he should have formed so beautiful a theory, but to me it is almost incomprehensible how he could have formed any at all. His was one of those original minds, stamped with the highest, most far-seeing faculties, and had it been cast in a university, and subjected to its discipline, it is hard to say what limits would have placed a bar to his discoveries. His theory, now demolished, however, is but a small part of his valuable contributions to the cause of science.

In Leydon, the celebrated Professor MUSCHENBROECK, with a friend named CUNEUS, and a monk, KLEIST, pursued their labors together on subjects which were attracting so much attention, and in the course of their studies they fell upon a discovery of remarkable importance, and the circumstances attending it were of profound interest. The effect produced by the passage of a spark from an electrified body was understood to be a pungent, slightly painful sensation. It occurred to Muschenbroeck, or one of the three, what effect would be produced if a stronger influence could be obtained; if a spark produced this result, what would be the effect of a power ten, twelve or twenty times as great. But the question arose how to try the experiment; with philosophical acumen they judged that if they could contrive to get a good recipient of the electricity, and manage to insulate it, they might then accomplish their object. But how should this be done? One of them said that water was a good recipient, and glass will guard it, because the fluid cannot pass through glass. "Now," said they, "why not pour water into a glass jar, and impart the electricity to the water? it must remain there—the water will be charged, and we will get a shock a hundred times as great as usual." This was all just and philosophical, and on this reasoning they proceeded. The jar was partly filled with water; Muschenbroeck took it into his hands; a metallic wire was let down into the water, by which the electricity was communicated to the fluid. Muschenbroeck, holding the jar in one hand after the water was charged, touched the metallic wire with the other. He received a shock from which he said he suffered more than from any pain he ever before experienced; and he declared that for the whole kingdom of France he would not repeat the experiment. He was even thrown into convulsions, and nothing could ever induce him to try it again.

The astonishment which filled all Europe when this was published was unqualified. Philosophers hardly dared to repeat the experiment, but gradually they they became reassured, and M. Allamand did repeat it. His sufferings, in consequence, were quite equal to those of Muschenbroeck: he declared that he should lose the use of his arms; he was seized with convulsions and a violent bleeding at the nose, and suffered in various other ways. Winckler was the next to try the experiment, and he suffered in the same manner. With a mixture of the curiosity of her sex and the courage of ours, Madame Winkler next attempted it, and was seized with the same pains. Her curiosity, however, soon got the better of her fears, and she repeated the trial, with the same results. We know now that the cause of these sufferings was nothing else than fear and the imagination. These experiments soon spread all over the civilized world, and soon passed from the hands of philosophers to those of charlatans and jugglers.

Meanwhile the philosophical inquiries were proceeding, and it is interesting to note the struggles with which Truth comes into the world—the throes and pains with which the infant first gets its being. The theory of the Leyden phial was soon explained to depend upon the presence of the two fluids. The inner and outer coats are separated by the glass. Suppose the wires connected with the inner coating to be touched to a conductor charged with *positive* electricity; the positive fluid goes to the inner coat and exerts an attraction and repulsion upon the outer coating. This is a point of the last importance, and I must ask for it your closest attention. The outside of the jar and the hand in contact with it are in their natural state; the positive electricity on the inside drives off the positive contained on the outside, through the hand and person, to the earth, while it attracts toward itself the negative. This is the physical law, for which we can give no reason. If you ask why it is so, I can only say, that you know as much about it as I do or any one else. It is one of those ultimate facts which depend upon experience only. The consequence of this action going on is, that the out-

side coating is charged with *negative* and the inner with *positive* electricity. The glass will not allow them to combine; it stands like a sentinel with fixed bayonet to prevent their union. If a connection be formed in any way, the two fluids will rush together. Now, Muschenbroeck held the outside in one hand and touched the wire with the other. The instant this was done a connection was formed, and the fluids rushed together through his body. All the mighty effects experienced by this shock, it must be remembered, were produced by a jar not larger than a common tumbler. Muschenbroeck never suspected that his hand had any thing to do with the effect; he held the jar in his hand merely for convenience. Now mark the curious slowness with which the truth was developed.

When the experiment was first known, a certain Dr. Bevis, a most acute electrician, less known than he deserves to be, gave his attention to the circumstances attending it. He observed that when the whole of the external surface of the jar was wet no effect was produced. He suspected immediately that the hand had something to do with the result, and began to consider whether it did not act as a conductor. He first found a person with a larger hand, and found the effect increased. He then wrapped a sheet of metal around the jar, thus virtually enlarging the hand; the effect was increased in proportion to the increased surface. This showed conclusively that the hand only acted as a conductor. The next question was, what office did the water inside perform? Was it a recipient of the electricity? If so, twice as much water should give twice as severe a shock—which was not the case. It was plain then that it performed no other part than to carry the influence to the inner surface of the jar. He then coated this inner surface, and his experiment was completely successful. He then went a step farther and ascertained that the form of the glass was unessential; for metal being laid upon each side of a common pane of glass, the experiment was as successful in this case as in the other. These developments were the work of considerable time, and involved the experience of the first minds of the day. So difficult was it to find this single grain of truth.

These experiments at Leyden soon reached London and were communicated to Franklin, who repeated them and endeavored to explain the results by his theory.

[Dr. LARDNER here announced that for lack of time he should postpone the conclusion of the subject of ELECTRICITY, until the next evening, and then, after an intermission of fifteen minutes, proceeded to remark as follows, upon]

THE SUN.

The subject from Astronomy, Ladies and Gentlemen, which I have chosen for this evening's Lecture, is *The Sun*—an object than which there is none more familiar, and about which there is yet such a lack of correct knowledge. This ignorance, if I must use an unpleasing term, is by no means confined to popular assemblies, since it is only ten or twelve years since the most remarkable circumstance concerning the Sun, and the one which first strikes the attention, has been known even by philosophers. You are all familiar with the observations of persons but little versed in astronomy, that the Sun is nearer to the earth in winter than in summer—a truth easily explained by the fact that the earth moves in an oval orbit, the Sun being in one of the two foci. The earth passes through that portion of its orbit which is nearest the Sun during the winter months; and as the distance diminishes, it is clear that the Sun *in a given time* imparts more heat in winter than in summer. The reverse will evidently be the case in the other hemisphere. This subject was not investigated thoroughly until about ten years since. POISSON, a French philosopher, closely sifted the question, and has shown that the heat does not vary merely with the change of distance, and that there is no more heat imparted in winter than in summer.—This fact remained unexplained until 1830, so that we may see that whatever reproach attaches to ignorance on this subject, philosophers are by no means free from it. Poisson said—true the earth is nearer the Sun from autumn until spring than from spring till autumn; therefore *in a given time* the Sun will impart more heat during the former than the laster space. But it must be borne in mind that the earth takes a shorter time in passing around its winter than its summer circuit; and although the heat *per diem*, is greater when the earth is nearest than when most distant from the Sun, still the number of days is less; the quantity given during one segment will be greater than during the other, because the number of days occupied in passing is greater.

Poisson, by an ingenious mathematical analysis, showed that these two influences compensate each other; that the increase of heat caused by the diminution of distance is corrected by the acceleration in motion. Thus time and space mutually compensate each other; and it is a mistaken notion that more heat is imparted to the earth in winter than in summer; the amount being just the same.

Now the real source of greater heat during the summer arises, not from any change in the distance, but from this: that the rays fall more perpendicularly upon the surface at one season than at the other. If the rays of light be supposed to fall upon a hard board placed obliquely to them, the surface will receive only a portion of the rays; while if it be placed in a direction perpendicular to that of the rays, it will receive the whole. The same holds true of heat; and carrying it out with respect to the earth, we see that during the summer the north pole leans toward the Sun, and his beams fall nearly perpendicular upon the surface; in winter the pole is turned away, and the rays fall obliquely.

This principle explains why there is more heat in summer than in winter. There is one other point of some interest in a popular explanation of this subject. You would naturally suppose, from what I have said, that the day when the Sun's rays fall in a direction exactly perpendicular upon the earth must of necessity be the hottest day. If perpendicularity and proximity be the sole causes of variation in temperature, then the hottest day should be the longest—the 21st of June. But this is not so: the hottest days do not occur

until the *dog days*—considerably later than the 21st of June. This, then, appears incompatible with the principle just stated. This point requires explanation, which may easily be given.—The heat of the weather depends not altogether on the obliquity of the Sun's rays. The heat on *a given day* depends on its obliquity; but if we take the earth from sunrise one day to sunrise the next, we find that during the day the Sun deposits upon the earth, and all objects upon its surface, a certain amount of heat, which, during the night, is more or less completely dissipated. As in midsummer the nights are shorter than the days, the loss during the night is not so great as the acquisition during the day; so that during the twenty-four hours there is, on the whole, an acquisition of heat. With this acquisition the next day commences, during which the same process is gone through with, and another certain amount of heat is acquired during the day over the loss during the night. The same thing occurs during every day while the nights are shorter than the days. We thus may keep a debt and credit account between day and night—the balance being in favor of heat. This would only continue while the loss during the night was less than the acquisition during the day; and after the nights became longer and the days gradually shorter, this would come to pass, that the heat lost during the night would just equal that deposited during the day, and our balance would be nothing. Then would occur the hottest day, and this takes place during the dog days. The same reasoning would evidently apply to the coldest of the winter months. The 21st of December, which is the shortest, is not the coldest day. The degree of cold is equal to the excess of the loss of heat during the night over the increase during the day. The balance is thus in favor of the cold.

We now come to consider the Sun, concerning which I shall have many observations to make. The physical constitution of the Sun is only known by careful observation, and you naturally ask how it can be examined, since its splendor is so intense as utterly to destroy the unprotected organ of vision. There is nothing more easy than to protect the eye by a piece of colored glass. With this protection the Sun may be submitted to the closest scrutiny. The first question which suggests itself is evidently to get at an estimate of its brightness. If we wish to obtain an estimate of its brightness to an observer *at* the Sun, we may suppose the whole heavens to be an immense Sun—from the zenith to the horizon covered entirely with the solar surface. The light would then be equal to the light of the Sun to a person placed on its surface, and the proportion between that light and the Sun's light to us would be the same as the proportion between the magnitude of the solar firmament we have just supposed and our Sun, which is as 300,000 to 1.

When we examine the Sun through a telescope, we behold *spots* upon its surface, some of which are exhibited in this drawing. [A splendid telescopic drawing of the Sun was here exhibited.]—These are liable to great and rapid variations. Sometimes the sun is seen without spots, and at others their number is small; they enlarge and diminish, and seldom last more than sixty or seventy days, and usually not so long. These spots are discovered chiefly along the horizontal diameter of the sun, and never more than thirty degrees above or below it. If they be watched from day to day, they will be seen to shift their positions, disappearing on one side, and reappearing on the other. Thus we know that the sun revolves on its axis, and its time is known to be twenty-five and a half of our days.

A careful examination of these spots shows their variable character. We shall find that they diminish in magnitude until, after a time, they entirely disappear; others come in their places; and thus they come and go, augmenting and diminishing with more or less rapidity. As we know the diameter of the sun, we can easily calculate the size of the spots; and it has been found that many of them cover a surface four or five times as great as the whole surface of the earth. Let me observe here that these spots are easily seen by ordinary telescopes; all that is necessary to guard the sight is to interpose between the eye and eye-glass a colored or smoked glass.

The number of spots to be seen varies greatly at different times, the surface of the sun being sometimes largely covered. The most remarkable case on record is that which commenced A. D. 535, when it is recorded that for fourteen months the light of the sun was greatly lessened, so as to cause a perpetual twilight, by reason of the spots on its surface. In 626, it is recorded that the sun was half covered. In 1779, and again in 1795, immense spots of from 60,000 to 70,000 miles across were discovered. On the 28th of August, 1805, a remarkable appearance was noticed; ten spots were seen to coalesce and form one enormous black spot on the sun's surface. It is found that in disappearing, the edges of the spots approach their centre at the rate of about a thousand miles a day.

Now the questions of most interest connected with this subject are, what is the physical constitution of the sun, and what causes the spots? Several suppositions have been advanced. Laplace supposed that the surface of the sun is covered with volcanoes, and that the solar light is nothing but the splendor of these volcanic fires thrown to the surface, and that the spots are *craters*. Sir William Herschell advanced a theory, the result of more careful study, and based more upon facts than speculation, which, moreover, is now generally received. Herschell supposed the sun to be an opaque, non-luminous globe, giving of itself no light; that it is surrounded by a thick atmosphere of flame which floats upon it as the ocean or air upon earth. If we suppose our atmosphere to be flaming fire surrounding the earth, it will represent what Herschell supposed the atmosphere of the sun to be. He accounts for the spots by supposing them to be the naked ground of the sun, seen through openings or cavities in the atmosphere, having sides sloping downwards. Their disappearance, he believed, was caused by the flowing in upon them of the fiery ocean by which they were surrounded. Accurate observation has confirmed these conjectures.

If we suppose the spots to be hollows and the edge a shelving surface, when it comes to the edge of the sun, by reason of its convexity, the eye is directed along the surface so that the penumbra on the inner edge will, in artistical lan-

guage, be *foreshortened.* This is the fact, as may be determined by observation. When it comes to the edge it will disappear. But we have a still more decisive proof that these are hollows. When they come to the edge of the sun they are seen sideways, of course, as the projection of light conceals the bottom. They must then disappear, as is actually the case. The assertion of Herschell, then, that they are cavities, acquires the character of a demonstration.

It is not necessary to assume, although these spots appear black when seen through a telescope, that they are really non-luminous. The eye judges by contrast. If we behold a very luminous object, and near it another considerably less brilliant, the latter may appear almost opaque in comparison. There is a light which has been called the oxyhydrogen light, composed of a ball of quicksilver excited to a flame by the oxygen and hydrogen gases. This ball when heated as much as it is possible for natural bodies to be, becomes so luminous that the eye cannot bear to look upon it. Such lights are often used as signals in great surveys; and I have known a case in which one of them on the top of a mountain 40 miles distant was distinctly visible, although the mountain was enveloped in mist. Now mark this: this ball of fire—so very luminous—has been held between the eye and the sun so as to be seen projected on the sun's disc; and when so seen it *appeared intensely black.* Not that there was any change in its absolute splendor, but the eye contrasting it with the greater splendor of the sun, the retina became insensible to that which before it could not behold without pain. This may convince us that these solar spots, which appear dark to us, may, in reality, be as bright as this oxyhydrogen light.

LECTURE II.

ELECTRICITY.

LADIES AND GENTLEMEN: Truth, whether contemplated in its own pure essence and for its own sake, or for the attainment of convenience and utility in the economy of life, is always an object of respect and admiration. But it is entitled to our highest reverence when viewed by itself, and separated from all considerations of utility. From the moment FRANKLIN first engaged in electrical inquiries, notwithstanding the respect for truth and enthusiasm in its discovery which shine through his life and writings, his views were constantly directed to the discovery of some useful purpose to which his discoveries might be applied. In one of his letters to COLLINSON, which is quoted in the Boston edition of his works, with whose editor, Mr. JARED SPARKS, I am proud to have formed a recent acquaintance, he expresses, in a playful mood, his extreme chagrin that thus far the great principles he had discovered were applied to no immediately useful purpose.—One passage of this kind I will read to you, in which he describes a projected party of pleasure to the banks of the Schuylkill as the hot season approached, when electrical experiments were extremely unpleasant: "Spirits," says he, "are to be fired by a spark sent from one side of the river to the other through the water; we shall eat upon *electrified* plates a turkey killed by an *electric* shock, and roasted by the *electrical* jack, by a fire kindled by the *electric* sparks; and we shall drink the health of all the *electricians* of Europe in *electrified* wine, from *electrified* glasses, under the discharge of guns from the *electrical* battery." Such was the character of the man and his mind—always bent upon making his singular discoveries useful to his fellow men; and he never could be quiet—he never could be perfectly happy until he had made these phenomena contribute to the comfort or the pleasure of those around him.

In the latter part of 1749, after bestowing much care upon the analogies on which his opinions were founded, he addressed a letter to COLLINSON, in which he maintaincd—and not merely conjectured, as has been asserted—that lightning and electricity were one and the same. In proving this, for an original, uneducated man—one whose mind had not been taught in the logic of the schools which renders the reasoning powers so acute, he proceeded with astonishing logical rigor. He said that the only way in which two physical powers admit of comparison is by their effects.—Now lightning and electricity agree in nearly all their general relations; electricity when it passes through the body of an animal in sufficient quantity, destroys its life or renders it blind, so does lightning; electricity fuses metals, so does lightning; it reverses the polarity of the needle and so does lightning; it fires combustible materials, and so does lightning; electricity, in fine, passes off by preference upon points of metal, while lightning was known usually to strike mountains, and high trees or spires. In short, he went through a whole catalogue of ten or twelve remarkable properties, and showed that electricity and lightning were identical in them all. He said that no doubt remained that the principles were the same, and being so, he saw no reason why we might not learn to deal with this fiery meteor from the manner in which we manage electricity. This assertion he threw out in a letter to COLLINSON, but he did not stop here; and the manner in which he proceeded to carry out the various modes of rendering the electric properties useful, are exceedingly interesting and instructive.

It was before known that electricity was attracted by points, as the discovery had been made by FRANKLIN. Now, said FRANKLIN, if the cloud which is charged with lightning be considered as charged with electricity, I see no reason why we may not erect a pointed body so as to draw the fluid from the cloud. He took a pair of metal scales, and suspended them by silk cords. Now then, said he, if the scales be electrified, the fluid cannot escape, and they may represent a cloud charged with lightning. He then laid upon the table a metal punch which was to represent the building—placed in such a position that the scales might pass directly above it. Now, said he, the same phenomena should be exhibited as in the case of an electric cloud. He passed the scales over the punch, and as it passed it slightly descended until within 'striking distance,' when a spark passed between them, and the scales rose and passed on. Here he had an image of an electric cloud passing over a building and discharging its lightning. Franklin then varied his experiment so as to illustrate the manner in which he proposed to render his discoveries serviceable to man.

He took a pointed needle instead of the blunt punch he had before used, and placed it so that the scale might pass over its point. This done, the scale was passed above it, when the scale instead of discharging its electricity, passed on its way, the fluid being stolen off by the pointed needle. Thus, said the philosopher, why may we not place metallic points upon the tops of buildings? and so he went on to his great discoveries in the manner so familiar to you all, and which, therefore, it is needless to repeat. He described an ordinary lightning rod, and went so far as to say that their points should be gilt to prevent their oxidation—a remark which would be natural enough in a man of science, but which at that time, and from such a man, evinced a most wonderful sagacity.

This occurred in 1750, and Franklin immediately issued a letter which was circulated in England, France and all over Europe, in which he described the method of drawing electricity from the clouds. He directed that a sentry box be placed upon a high elevation, and that from this a pointed rod should ascend into the air. This point would attract the fluid, a person in the sentry box could easily perceive its presence by the usual tests, or charge a phial and produce a shock.—These were his directions, and the account containing them was read before the Royal Society of London, and is said to have been received with peals of laughter as the dream of a visionary enthusiast. The matter thus remained until August 1752. Franklin had an idea that it was necessary to raise the rod to a great elevation, and waited a long time for the completion of a high spire at Philadelphia. But the notion occurred to him after issuing his directions to the philosophers of Europe, of trying his experiment in the manner with which you are all acquainted—by sending into the air a kite armed with a metallic point. He determined upon this course, and in the early part of June, 1752, tried his experiment upon the common at Philadelphia, the details of which I need not recount, as you are all of you familiar with them.

This, then, was the course which Franklin took to prove the identity of lightning and electricity, and it is not until recently that his claim to the discovery has been disputed. But in 1831, M. Arago, the Astronomer Royal of France, whom I have so often mentioned, and whose acquaintance I have the honor of enjoying, published a beautiful memoir or *Eloge* of the great electrician Volta. In this *Eloge* he took occasion to refer to the circumstances of the discovery imputed to Franklin. Franklin's experiment to ascertain the identity of lightning and electricity he said was useless, for it was sufficiently established by the flames which appeared on the spears of the Roman soldiers of the Fifth Legion mentioned by Cæsar, The experiment therefore, he said, was not called for, since it gave no knowledge not already possessed. Franklin's suggestion of their identity, moreover, he said, was a mere conjecture, and such as it was, he had been anticipated in it by the Abbé Nollet, who had made the same suggestion in a work published in 1749. Thus, in the view of Arago, Franklin was not only deprived of the merit of the experiment but of the discovery itself. This passage in the Eloge caught my attention while I was in Philadelphia, and I then saw the original works which Franklin used and which contained his manuscript notes. I sifted the whole question, and here are the results. The work of the Abbé Nollet containing this suggestion was published at Paris in December, 1749. The hint was a mere conjecture affording none of those analogies by which Franklin tested and established the identity of electricity and lightning. But suppose Nollet's declaration had been explicit and his reasons conclusive; it is scarcely possible that a scientific work published in Paris in December, 1749, should have reached Philadelphia as soon as the date of Franklin's letter to Collinson which first announced his discovery.

Let us now see how stands the experiment.—Arago maintains that the credit of this is due not to Franklin, but to a French electrician named Dalibard, who, Arago says, made the experiment at Mary-le-ville, near Paris, about a month before Franklin had gone through with his. This is all true so far as the mere fact of the experiment's having been tried at the time is concerned; but let us see the circumstances under which M. Dalibard's experiment was made. In 1750 Franklin issued a letter in which he requests the attention of electricians to the identity of lightning and the electric fluid. He describes the apparatus by which the question may be settled: directs the erection of a sentry-box and metallic rod, and details minutely the whole process by which the experiment may be made. This letter goes to the Royal Society of London, is printed, translated into French, goes to Paris, and has a general circulation all over Europe. It falls into the hand of Dalibard, who himself did not dream of the honor which after sixty or seventy years was to be conferred upon him. He determined to try the experiment, and made his preparations in strict accordance with the directions of Franklin. He was accustomed to spend the most of his time in Paris; and wishing to be in that city he gave his experiment into the hands of one Coiffier, a carpenter, to whom he repeated the directions given by Franklin. He left the whole in his hands, and returned to Paris. While he was there, there arose a thunder-storm, during which Coiffier practiced the directions laid down, applied his knuckle to the butt of the rod, and received a spark.—Forcibly struck with this phenomenon, he flew to the village to find the *curè*, as he was a man of reputed learning and science. The *curè* came, the experiment was repeated in his presence, and he immediately despatched a *process verbal* with a full account of the manner to Dalibard at Paris. Dalibard immediately sent it to London, where it was published in the Philosophical Transactions precisely as it has been related. In this account it was mentioned as the experiment of Franklin, performed at the expense of Dalibard by the carpenter Coiffier, and that it was entirely successful. With this plain statement of the facts, you are all in a condition to form a judgement, as well as I or M. Arago himself can do it, how far the merits of the discovery is due to the one or the other.

Another discovery, for which Franklin has not received proper credit, is that of the principle of *induction*. Having so much before me upon which I am expected to lecture, and having, per-

haps somewhat imprudently, announced a great variety of subjects, it will be necessary for me to pass rapidly over the ground laid out. I can therefore only briefly notice the discovery of this principle, the nature of which will be evident from my remarks. Suppose a brass cylinder be suspended by silk threads. At one end suspend, by a moist silk card, a silken tassel. The cylinder and tassel are both in their natural state.—Franklin found that, if he took a glass tube, which had been electrified, and held it *near* the extremity of the cylinder, the opposite end became electrified, without being touched by an electrified body. The fact was known by the divergence of the threads of the tassel. Franklin explained this in accordance with his own theory of a single fluid thus: he said that when the electrified tube was brought near the end of the cylinder, it drove off its natural electricity to the other end—and that the fluid took refuge in the tassel and caused its divergence. When the glass tube was withdrawn, the fluid rushed back and was again spread over the conductor. This is the principle of induction; that the electrical state is produced by simply bringing an electrified body near to one in its natural state. It is true that Franklin's mode of explaining this, based as it is upon his own theory, is not generally received. And here let me say, that perhaps I asserted too positively last evening that Franklin's theory of electricity was obsolete: there is a section of the philosophical world which adheres to the theory of a single fluid. But I believe I am correct in saying that the mathematical section of the world adopts the opposite theory of two fluids from the impossibility of explaining by Franklin's all the circumstances that occur. Franklin, beyond all doubt, is entitled to the merit of having discovered the principle of induction—one of the most important in the whole history of science—fully equal to that of the principle of conductors and non-conductors.

I must now pass on to another discovery in electricity of vast importance. There lived in Como, in the Milanese, an individual, at that time obscure and unknown, but since most distinguished, named VOLTA. This philosopher, so eminent, directed his attention at an early period to the subject of electricity, and especially to atmospheric electricity. Franklin had observed that the atmosphere, where it was clear and free from clouds, was always charged with positive electricity, and to this he found no exception. It is true that when the air is filled with clouds its electricity is sometimes positive and at others negative—depending upon the position and relations of the clouds. But when serene and unclouded, it is always charged with positive electricity. The natural inference from this fact is, that the earth must be charged negatively; for that electricity which resembles the earth's would of course be repelled from its surface; but we find that the positive is not repelled, since it remains in the air; therefore the electricity of the earth must be negative. This fact, until the time of Volta, remained naked and unexplained. He pondered upon it long and earnestly, and the result of his labor was a discovery of vast generality and great importance. He concluded from long consideration that the positive state of the air must arise from a process of universal evaporation continually going on upon the surface of the earth. He saw at once what a vast discovery he should have made if he could prove that the same process which carried moisture into the air, again to descend in the form of rain and dew, was also the agent which gave birth to the lightning, the aurora, and the whole class of electrical phenomena, which till then had been unaccounted for. He felt an intense and instinctive conviction of its truth, but for some time saw no way of trying the experiments, by which it was to be established. At length he hit upon a method, which he carried through with complete success. The experiment was this: he procured a shallow metallic vase containing a quantity of water. The surface being large, a process of evaporation was continually going on. He placed this upon an insulating stool, and attached a metallic chain to one ear of the vase. This chain conducted to an electroscope. [I may here explain that an electroscope is an instrument used to detect the presence of minute quantities of electricity. It consists of a rod passing into a glass jar, at the other end of which are two slips of gilt paper, the whole being protected from the air by the jar. If the slightest possible amount of electricity pass through the rod to the leaves, it being of the same character in each strip, these will diverge, and thus the presence of the fluid may be detected.] Now I request your particular attention to this beautiful experiment. The vase, said Volta, represents the earth; the water in it the liquids upon the earth's surface. Now, if evaporation be the cause whereby the atmosphere becomes changed positively, this I am sure of: that the evaporation of the water in the vase, carrying off the positive electricity, will leave the negative in the metal of the vase. That cannot escape, for the vase is insulated; failing to escape, it will pass along the chain to the electroscope, the leaves of which will diverge, and thus I can detect its presence. Thus the leaves became *tell-tales*—indicators of the presence of electricity in the vase. Volta made the experiment, and the result was just what he had anticipated. As soon as the evaparation had proceeded sufficiently, the leaves diverged, and on testing its quality he found that the electricity was negative. This was a discovery of the first order, and the first which surrounded the name of Volta with the celebrity which was still farther increased by his invention of the Voltaic Pile.

Let me here observe that the most sagacious philosophical minds often fall into practical errors; Volta, in fact, was wrong when he said that *mere* evaporation was the source of the positive electricity of the air. True, the evaporation of the water in his experiment produced this effect, and it is also true that in a qualified sense, evaporation is the main instrument in maintaining the positively electrical state of the atmosphere. Experiments conducted by more acute minds than Volta's have since proved that if the water be perfectly pure—if it be distilled, and all matter held in solution by it be expelled, no electricity will be evolved by evaporation. The fact is, that all water on the earth's surface holds in solution various salts or earthy matter; and the presence of these particles is absolutely necessary to the evolution of electricity by evaporation. Volta was right in

the main fact, and his discovery evinces a wonderful sagacity, and was of a high order; and for it Volta has received deserved credit.

GALVANISM.

I now come to notice another branch of this subject, in which will be found many phenomena of a highly interesting character. M. Arago, whose writings are so attractive and deservedly popular, and whose works cannot be too strongly recommended to all the admirers of delightful ease in scientific writing, remarks that the grand discovery of Galvanism can be distinctly traced to the fact that a Bolognese lady, having a catarrh, was ordered by her physician *frog broth* as a diet.—Galvani was Professor of Anatomy in a University at Bologna in 1790. Mad. Galvani chanced to be afflicted with a cold in the head, for which was prescribed a broth prepared from certain portions of a frog. Her husband had been performing some electrical experiments, and the members of the frog chanced to lie upon the table near his apparatus. Mad. Galvani was astonished to observe, as a pupil of her husband was performing some slight experiment, the limbs of the frog start and be convulsed. She called to the fact the attention of Galvani, who repeated the experiment, and found a motion resembling that of life in the dissected limbs of the frog. Immediately Galvani, who was not an electrician, but a physician, was seized with the idea that he had made a grand discovery; he believed that he had found some electrical principle in the animal economy, and having once got this idea in his head, he consumed years of his life in following it out, and endeavoring to make it clear.

It is a most curious fact that the discovery of galvanism owes its origin to the ignorance of Galvani. If he had been well instructed, he would at once have seen that the reason of the convulsive movement in the limbs of the frog was owing to nothing else than what is called the *lateral shock*; which is the shock produced by passing an electrified conductor *near* a body in its natural state. When the machine was electrified, a shock passed near the nerves of the frog—which are the most delicate electroscopes—and caused the convulsive movement that he observed. Had Galvani seen this, his inquiries would have stopped here, and he would not have fallen upon his subsequent discovery.

Some time after he had first observed this phenomenon, he dissected a frog for the purpose of repeating the experiment; and in order to prepare the limbs, he found it necessary to hang them up for a short time. He took a copper wire, made a hook at each end, and passed one hook through the nerves of the frog, and hung the other upon the iron of the balcony. To his astonishment the nerves soon began to play as if alive. This attracted his attention, and led ultimately to the discovery of galvanism. He accounted for the phenomenon in this way: He looked upon the metal only as a conductor of the electricity of the nerves and the muscles of organized bodies. At the junction of the nerves and the muscles he thought the electricity was evolved—the one kind passing along the nerves, and the other along the muscles. Now, if you take a wire, and attach one end to the nerve and the other to the muscle, a communication is thus opened, and the electricities may be united by passing along the wires; thence, said Galvani, arises the convulsive motion. This mode of accounting for it was for some time in fashion, and was generally received by scientific men.

At this time Volta, who performed the experiment I have already described, had commenced his observations at the University of Pavia, not far from Bologna. Hearing of Galvani's discovery, he applied to its consideration higher powers of mind than Galvani possessed, and proved that the latter's supposition of the origin of electricity at the junction of the nerves and muscles had no foundation whatever; and he proved, moreover, that the evolution took place in the two metals; that when the iron and copper came together, electricity was evolved by their mere contact.—Thus Volta saw nothing wonderful in Galvani's experiments, except that a new sort of electricity was discovered, and gave a variety of reasons to disprove Galvani's elaborate and wonderful conclusions. Volta soon after followed up the inquiry, and in the year 1800, a year rendered further remarkable by the discovery of the small planets Vesto and Ceres, discovered one of the greatest instruments of philosophical investigation—the *Voltaic Pile*.

He first discovered the fact that by bringing together different metals, as zinc and copper, electricity was evolved. He soon saw that by a little ingenuity these metals might be multiplied to any extent, and that thus he might obtain a constant stream of the electric fluid from sources never before suspected. He reasoned in this way: Suppose I place upon a plate of copper a zinc plate: their contact will cause a continued stream of electricity—the positive passing along the zinc, and the negative along the copper.

Now, if the extremities of these two metals most remote from their mutual contact be connected by a metallic arc, the positive fluid will move from the zinc to the copper, and the negative from the copper, toward the zinc, along this arc. And again, if I can place upon this pair of plates another pair, likewise evolving electricity, and over these a third, it is evident that currents of electricity will be established at each surface of contact of the two metals, the positive current running along the zinc, and the negative along the copper.—There will then proceed from the first surface a downward negative and an upward positive current: from the second, a downward positive and an upward negative, &c; the downward current being negative and the upward positive from the upper surface of each copper disc, and the upward negative and the downward positive from the lower surface of the copper disc. Both the downward and the upward currents will then be alternately positive and negative. Some expedient must therefore be adopted to render all the currents in the same direction of the same kind; then the current issuing from the bottom of the pile would be a negative current as many times greater than the one from the single pair as there are surfaces of contact supplying the currents. Volta selected discs of moist cloth to place between the several pairs, since these would prevent the action of all the surfaces in which ascending negative and descending positive currents could originate, and at the same

time act as a free conductor of electricity. His apparatus then presented, first copper, then zinc, then cloth—copper, zinc, cloth, and so on as far as he chose to carry it. This he called the Voltaic pile, which afterwards assumed a new form in the Voltaic battery.

Volta soon saw the importance of this discovery, and wrote to Sir JOSEPH BANKS, then President of the Royal Society of London, describing his invention; and so conscious was he of the importance of it, that he did not wait until it was completed, but wrote, before he had finished it, so much as he had already ascertained. Sir Joseph received the first half of the letter early in March, and the other in June, of the year 1800. It was immediately made known to Sir ANTHONY CARLISLE, and to Mr. NICHOLSON, known as the editor of 'Nicholson's Journal;' and they at once constructed a pile in the manner described. The first fruits of their experiments were the discovery of the decomposition of water into oxygen and hydrogen gases. This subject received general attention in England; and just at this time Sir HUMPHREY DAVY (then Mr. DAVY) was commencing those discoveries which have surrounded his name with so much lustre. Some intelligence of these experiments fell into his hands, and the result was a number of still more important discoveries.

Meanwhile the news of Volta's experiments reached Paris and came to the ears of NAPOLEON, whose patronage of science and of all liberal arts was as munificent as his military triumphs were splendid and imposing. The First Consul immediately sent to Italy and invited Volta to visit Paris to exhibit his experiments before the *savans* of that city, that they might hear from his own lips the mode in which he had pursued the subject. Volta came and spent three or four days in lecturing to the French Institute upon the details of the subject. The effect produced upon dead bodies by galvanic action, now so well known, was there exhibited, and excited universal astonishment. There has lately been published an observation made by Napoleon while witnessing these experiments: turning to Corvisart, his physician, he mentioned the striking analogy between the phenomena of Galvanism and those of life, and went on to show how the vertebral column might act as a voltaic pile, and to trace the analogy through different parts of the system. This occurred in the presence of Chaptal, and was first published by Bequerel, to whom Chaptal told it. In 1830, Sir JOHN HERSCHELL made an observation in his Preliminary Discourse on Philosophy, bearing a striking analogy to that of Napoleon, without any knowledge of the above anecdote.—He says—"If the brain be an electric pile, constantly in action, it may be conceived to discharge itself at regular intervals along the nerves which communicate with the heart, and thus to excite the pulsation of that organ. This idea was forcibly suggested by an elegant apparatus, the dry pile of DE LUC, in which the successive accumulations of electricity are carried off by a suspended ball, which is kept in a state of regular pulsation for any length of time." Another circumstance, still more remarkable, is, that Dr. ARNOTT, a Scotch philosopher living in London, published a work on Physics, in which precisely the same idea is suggested. It is a curious fact that these three minds should all have fallen at the same time upon so remarkable a conjecture.

And now, as I must trespass somewhat longer on your attention in this branch of the Lecture, I will mention one or two of the great discoveries which Sir H. Davy made in the course of his investigations. I have already stated that Carlisle and Nicholson had discovered that a wire passed from one end to the other of the pile conveys electricity from each end. This they followed out, and found that if the points of two wires connected with the two extremities of the pile be immersed in water, upon coming together they effect a decomposition of the water into oxygen and hydrogen gases; the oxygen being attracted by the positive, and the hydrogen by the negative fluid; and then being evolved, they separate and escape.—The first subject which attracted Davy's attention was this curious result, which to this hour is unexplained, and may be accounted an ultimate fact. He determined to try if decomposition could be effected in a water contained in separate vessels united by a conducting substance.

To try this, he placed in one vessel the positive wire and the negative in the other. Between the two he placed a third vessel of liquid, and formed a connection between this and each of the other vessels by a metal conductor like a syphon. As soon as the connection was formed, the decomposition was effected—the oxygen rising from the negative glass, and the hydrogen from the other. The question arose, what became of the hydrogen evolved at the negative pole, for they could exist only in combination? It did not appear, and it certainly could not remain in the water. Again, what became of the oxygen evolved at the positive pole? No one could tell, nor can any one to this day. It seems an inevitable conclusion, that when water is decomposed in one vessel, the oxygen passes into the liquid and escapes; or else it travels through the conducting body, and thus makes its escape. More wonderful still: he found that when he took away the middle vessel, and placed, instead of it, himself, placing one hand in each vessel, one of the two principals must pass through his body by some mysterious agency.—Various theories have been devised to explain this phenomenon—by a series of decompositions and recompositions; but none of them are satisfactory; and this naked fact alone is left resting upon experience for its basis—that the water is decomposed, and one of its constituents escapes at one vessel, and the other at the second. The theories of GROTTHCUS and the others who have written upon this subject are all inconclusive and unsatisfactory.

[The Lecturer here concluded the first part of his Lecture, and passed to the consideration of]

THE SUN.

I was discoursing, at the conclusion of the last evening's lecture, upon the circumstances and physical state of the surface of the Sun. There are some very striking facts connected with this subject, which have been but recently unfolded, and of which many are the discoveries of living philosophers. It was the opinion of the late Sir WILLIAM HERSCHELL, (and his supposition had many advocates,) that the Sun is an opaque globe,

entirely covered by a luminous fluid of some kind; but whether it be an ocean of liquid fire, like molten iron; or an ocean of gaseous fluid, as of flame; or yet again, an incandescent solid, like iron glowing with white heat, remained wholly without explanation until within a very recent period. But the question has now been reduced to a positive absolute demonstration; and the process by which done, and the circumstances attending it, afford a beautiful example of the mode in which the different sciences minister to each other.

We see, in this singularly elegant solution of an apparently insoluble problem, three sister sciences aiding and cherishing each other. In Optics, a beam of light is proved to be susceptible of a peculiar modification called *polarization*. All light is proved to be in either a polarized or an unpolarized state; and although it may not be easy fully to explain what is meant by the polarization of light, still by the aid of a fanciful illustration I can give you a notion of it sufficiently precise for our present purpose. Suppose this wand, which I hold in my hand, to represent a ray of light; it has four sides; suppose the two opposite sides, which are painted blue, to possess a certain property—and the other two opposite, which are colored red, to possess a property different from the first. This wand, then, exactly represents a polarized ray of light. If all the sides had the same properties, then it would represent an unpolarized ray. Now light may undergo a certain change which shall polarize it—imparting to two of its sides, opposite to each other, a certain property which the other two do not possess. The question arises, what are these properties?

They are various; one, however, is so simple and so nearly connected with the demonstration to which I call your attention, that I shall mention it. If a ray of light fall upon a reflecting surface with either of those *two* sides, which are represented by the red sides of the wand, it will be reflected at an equal angle to that by which it approached the surface. But if it strike the surface upon the other opposite sides—the blue—it will not be reflected at all. So that two of its faces are capable of reflection, while the other two are not. This is one of the qualities by which polarized light is characterized. In a ray which is not polarized, reflection takes place under all circumstances; but with polarized light, only under certain conditions. Thus we see that light may exist in two distinct states; the one unpolarized, or its ordinary state—and the other polarized, or its extraordinary state. Now this is the truth which has been contributed to this demonstration by the discoveries of modern optics. Let us turn to another branch of physics.

The science of heat has received more attention within a few years past than any other branch of physics. Fourier, a French philosopher, has done much in this department of knowledge. One of the conclusions he established is this: There are three states in which material bodies exist, viz: the solid, liquid, and gaseous. Fourier proved that when a *solid* body becomes incandescent, the light which it admits is polarized; that the light emitted by an incandescent liquid, as molten iron, is likewise polarized; and that the light of incandescent gases, as flame, is unpolarized. These facts are true, whatever may be the nature of the materials. Here is a distinction established by this great natural philosopher between light emitted by incandescent solids and liquids and that emitted by gases. This is the contribution from the science of heat.

Now M. Arago has, with most beautiful sagacity, availed himself of these two facts, contributed by the sciences of light and heat, to determine the nature of the sun's atmosphere. This may easily be done; for since it is established that the light from incandescent solids and liquids is polarized, all that need be done to determine this point is to try by experiment whether its light be polarized or not. Arago, by applying the usual tests, found that it is not polarized: the conclusion, as inevitable as it is important, is, that the surface of the Sun is covered, not by a solid or a liquid, but by an atmosphere of flame. Here is one of the most beautiful inferences drawn from the whole range of physics; and it is established by the aid of science, with all the certitude of a mathematical demonstration.

Arago proved, therefore, that the Sun's atmosphere is an ocean of flame. It had long been supposed that the Sun was surrounded by an atmosphere of light, and this opinion was mainly grounded on the assertion of an eminent French observer of the Sun, that its borders were less luminous than the centre; and the inference from this was justified by saying that a thicker portion of the Sun's atmosphere came between the eye and the centre than at the edges. This, however, has been proved by M. Arago not to be the case. There is a certain kind of crystal which presents a double image: this he employed in his experiment; and by casting the edge of one image of the Sun upon the centre of the other, he showed clearly that this assertion of the French astronomer was unfounded.

I shall now dismiss the subject of the Sun, although, did my limits allow, I might discourse volumes upon it, and give you a multitude of facts not less interesting than those I have already brought forward. So many other subjects, however, claim attention that I can proceed no farther with this.

THE FIXED STARS.

I now come to notice the Fixed Stars, and all that I can do to-night, without infringing on your time more than I dare to do, is to explain, as well as the mathematical nature of the subject will admit, the way in which we determine—not the distance of the Fixed Stars, for that no powers of intellect with our present data can do—but the distance from the Solar System less than which we know the nearest of them cannot be. This is a species of inquiry which may be made and in which we can say this: that the nearest Fixed Star is farther from our system than a certain nameable number of miles.

Before I proceed to exhibit the mode of making this calculation, let me say that there are certain methods of proving, with absolute certainty, that beyond the limits of the solar system there is a vast, immeasurable abyss of space in which there exists no material body. For our system, swinging in this boundless void, has been supplied by its Maker with *feelers* of the most delicate sensibility,

which reach far out into the empty space—far beyond the limits of vision and almost of thought—examine its whole extent with unerring skill, and enable us to say with absolute certainty, that there is within that abyss no mass of matter of any perceptible gravity. These *feelers* are—*the Comets.* HALLE's comet, as I have before shown, sweeps around the sun in an orbit reaching two hundred thousand million miles beyond the most distant planet in the solar system, which itself is two thousand million miles from that system's centre.—Halle's comet, then, when it swept past us in its orbit round the sun in 1759, and passed close to that luminary, traversed the distant orbits of Jupiter and Herschell, issued from the solar system and launched into the abyss of space, winging its invisible way two hundred thousand million miles beyond Herschell's far-off orbit. For seventy-five years it traversed the everlasting void; no eye beheld it in its silent journey; it went on unseen but not unknown; the finger of philosophy was upon it, the eye of the mind beheld it in its wanderings, traced its deviations, noted all its doings, and patiently waited its reappearance in 1835 near the constellation of the Great Bear—the precise spot and date assigned to it by the unerring laws of Science. Now, if in that great abyss of two hundred thousand million miles which yawns between the farthest limits of the solar system, and the nearest possible material body, there had existed any mass of matter, its power of attraction upon that comet would have been made known: causing it to deviate from its course and lengthening or shortening the time of its absence. What took place between 1682 and 1759? At that time CLAIRAULT calculated the goings on of this comet in the regions of invisible space and predicted its reappearance. He said, 'I know that it will appear upon that day. Beyond the bounds of Saturn's orbit there may exist another plrnet. If there be one there its influence will be felt by the comet and it will not return at the precise period I have named.' That planet did exist—its influence *was* exerted, and the comet did not return upon the day at first assigned by Clairault. But this comet has gone two hundred thousand million miles beyond that planet, and yet feels no other power. It neither anticipates its time nor lingers on its way. This comet, then, of Halle's is a swift-winged messenger sent out from the solar system to inquire whether there exists in that vast chasm any material substance. It has gone on its mission and returned, and it tells us *there is none.* Within the measureless universe our solar system stands alone; around about it is a vast and empty chasm in which no material body has a being.

Now let us see what are the calculations which tell us the limits within which the nearest Fixed Star cannot be.

You will all recognize the principle of the following calculation: if standing in this position I behold a distant object, as that chandelier in the distant part of the building, and then take its bearing; then if I change my position and again take the bearing of the object—and then measure the distance between my two positions, I have all the data necessary to calculate the distance of the chandelier. I have given all the angles, and one side of the great triangle: the calculation of the length of the other sides is extremely simple. Now I beg you to consider this: Suppose that I am not a man capable of changing my position thus: but suppose the intellect I possess to reside in the head of an animalcule, having his home upon a grain of dust upon this floor. Suppose the little creature to be reflecting upon that distant chandelier and curious to know its distance. This he proceeds to calculate: standing upon one side of his grain of sand he takes the bearing of the brilliant light; he travels over his earth—a distance perhaps of a thousandth part of an inch—and again attempts to take its bearing. He is astonished to behold that it is the same; the greatest distance he can possibly travel—the largest base he can get—the diameter of his grain of sand is so inconceivably small that he is utterly unable to get any new bearing and therefore to obtain any *modulus* whereby to measure the distance of the chandelier.

Our animulcule is Man—placed upon the Earth; the chandelier is the nearest fixed star. On the first of January he looks at the star and takes its bearing. He says: I know that the earth travels around the Sun in an orbit two hundred million miles in diameter. I know then, that on the first of July—six months from this time—I shall be at the vast distance of two hundred millions of miles from my present station. I will wait: and when I arrive there, with this vast distance for a base, I surely cannot fail to get a bearing which will give me the distance of the star. He waits until he arrives at that point—makes his observation, and what does he find? The *bearing of the star is the same!* The whole diameter of the earth's orbit, compared with the distance of the nearest star, nothing more than the diameter of the grain of dust compared with the distance of the chandelier.

Now mark this: though we cannot detect the difference of the star's direction from these two widely separated positions, still we know that it does exist; but it is smaller than our instruments can measure. But we know how small an angle our instruments are capable of measuring; we are sure that if the difference amounted to 2″ it could be detected. We have therefore arrived at this conclusion: the difference between the bearings of the star must be less than 2″. Suppose, then, that it is just 2″—which we know it cannot be; we can then calculate what would be the distance of the star. The earth is distant from the sun, in round numbers, one hundred million miles. If the difference in the bearings of the star, taken from the opposite side of the earth's orbit, be 2″, the distance of the star must be two hundred thousand times one hundred millions of miles, or 20,000,000,000,000 of miles. The nearest fixed star, then, we know with absolute certainty, cannot be within that distance from the Solar System. How much farther off it may be we cannot tell. Philosophers, as well as others, have found it difficult to form any adequate conception of these great distances, and they have been forced to devise help for the imagination in the attempt. The method usually adopted is this: Light we know moves at the rate of 200,000 miles in a second of time. According to the above calculation, light would require more than three years to pass from the nearest star to the earth.

Until recently no attempt had been made to ar-

rive at an approximate conception of the magnitude of the splendor of the fixed stars. In 1829, Dr. WOOLLASTON, to whom physical and especially astronomical science is so greatly indebted, attempted just before his death to form some notion as to their size and splendor. The particular star he chose for his observation was Sirius, or the dog-star—a star of the first magnitude, expressly bright, and to be seen near the constellation Orion. Woollaston made observations to determine the comparative splendor of this star and the sun. There are certain well known methods of ascertaining the comparative splendor of two lights, by a branch of science called the *photometry*, and the instrument employed is called *photometer*. Woollaston compared the light of Sirius with that of the sun, and found it to be 20,000 times less. Having established this, and knowing then the general principle that, if the sun be removed to twice its present distance, his light will be one-fourth of what it now is—if removed to three times its distance, its light will be one-ninth, &c,—it is easy to tell how far it ought to be removed in order that its light should be one-twenty thousandth part of what it now is, or that its appearance should be the same as that of Sirius.—Woollaston calculated that it should be removed to one hundred and forty thousand times its present distance from the earth. But it is quite certain that Sirius is very much more distant than that, and yet it gives as much light as the sun would in that position. The conclusion then is inevitable—that this dog-star is vastly larger and more splendid than our sun. The inference which Woollaston drew—and he was far from being sanguine in his character; his mind was acute, and he was rather skeptical than credulous—yet his conclusion was that Sirius is equal in size and splendor to fourteen of our suns. Consider then what vast views are thus opened for our contemplation. The nearest, and for aught we know, smallest of the mass of stars that inhabit the universe, is equal in magnitude to fourteen of our suns! How much greater it may be, or how it may compare with its thousand companions, no one can tell.

LECTURE III.

THE ARTS AIDED BY SCIENCE.

LADIES AND GENTLEMEN: When we look back at the progress of the human race in the arts of civilization, we are forcibly struck with the truth that the sentence of labor pronounced upon our first parents as the condition of acquiring material goods, was by no means confined to the lower and animal wants of man; but that it has the same imperious force as applied to the gratification of his higher intellectual necessities. We see, it is true, that Providence has supplied various means of advancement to the human race; but these are not thus perfect, as we find them, but are rather susceptible of receiving perfection from the application, not of mere human endeavors, but of those higher powers of intellect—the image of himself which God has stamped upon the mind of man. It is a remarkable fact that when we cast our eyes around our world, we find man the most helpless and defenceless of all the animal world. Some of the other animals are provided with coverings of wool, feathers, &c, to guard them from the vicissitudes of temperature; others have talons, claws, horns, or teeth, to serve as weapons of attack, and others are swift of foot, keen of eye, or quick of scent, to enable them to make good their escape from all that would annoy. In man alone, of the whole creation, do we find utter and total helplessness. Neither wool to protect him from the climate, nor feathers to guard him from the air, nor any of those defences with which other animals are furnished, belong to him. And yet we find this helpless creature king of the whole creation. How has he acquired this vast ascendency over all the animals so much more liberally endowed? By the simple power of that noble intellect which distinguishes him from all other created beings on the face of the earth. We see in every case where nature has imposed the necessity of exertion, it has been done just to that degree which provokes the operations of their intellectual power.

THE MAGNETIC NEEDLE.

In the early stages of civilization, when traffic between different nations was narrow and confined, navigation was almost entirely coastwise, and made principally to countries not far removed, and inhabited by people of not dissimilar habits. This property of the magnet was soon discovered, —that when suspended freely it directed itself to the poles: and this became an instrument by which the early powers of navigation were increased and the progress of civilization greatly aided and advanced. It was soon found, however, that this instrument, put by Providence into the hands of man, was irregular in its operation, and that it could be usefully employed only by application of the intellect. It became apparent that the magnetic needle not only did not direct itself strictly to the pole, but that it was not constant to any fixed point: that it was liable to continual variations depending upon its place and other circumstances. Still it was seen that it had a sufficient direction to be of essential service to the navigator; and the irregularity which had been observed only stimulated to action the faculties of the intellect.

It was found that it did not point due North; and soon after its first adaptation to the purposes of the navigator was discovered, it was at first supposed that the deviation from the true direction was fixed and permanently the same; and it was thought that if the amount of this variation could be detected, all that would be necessary to its correction would be known. But on transporting it to different places it came to be observed that its variation was governed by no known fixed law—but that it differed continually in different places, and that in using it as a mariner's guide, only a rough approximation to the true direction would be obtained. Natural philosophers long sought to lay down upon a chart these variations of the compass, for every place upon the globe, so that the variation for each place might be registered and thus for ever known. But no such chart was ever made of practical service; and resort was finally had to simpler rules by which from day to day the exact deviation of the needle from the true meridian might be determined. There are many of these methods

in common use, and for the sake of illustration I will point out one.

Suppose that the direction of the Needle, in a certain case, is west of the real north; the manner in which the mariner upon the trackless ocean would determine how far west it might be would be this; Certain rules are supplied him by which for every day in the year, he can tell the sun's declination—its distance from the celestial equator; his own latitude he can easily calculate. He has the means then of telling the point in the horizon at which the sun will rise—and can say how many degrees it will be on the meridian from the pole. Now suppose the angle of the sun's direction be 70° while the angle of the direction of the Needle from the Sun is 65°; he knows that the Needle varies 5° from the true north. Thus after making the correction he has the true north, which will continue the same while the Needle returns its variation, which will be more or less according to the position of the ship, the lapse of time, &c. This is laid down upon his log and carefully preserved, so that his direction may be always known.

The Magnetic Needle not only does not always preserve a constant direction toward the north, but when suspended upon a horizontal axis it will not hang in a vertical direction, but will *stoop* downward a certain angle. This fact was discovered early and is called the *dip*.

THE LATITUDE AND LONGITUDE.

It was for a while supposed that by observing the amount of the dip, the position of a ship at sea might be ascertained, as it was found that in no two places is it precisely the same. By laying down, then, its amount for every place, or by finding the law of its variation, it was supposed that the ship's position might be readily determined. But this method is only used by navigators when the sky is cloudy, when they have not instruments for other calculations, or under certain other seldom recurring circumstances. The observation of the stars is now regarded as the only true method for obtaining the position of a ship at sea, and to this discovery may be owing to a great extent the present high state of navigation. Nothing can be more beautiful than this method of determining a ship's position by astronomy; it is moreover easily capable of popular explanation.

In the first place I will endeavor to show the method of finding the distance of a ship at sea from the line—in other words, of determining its latitude; and next the way of solving the far more difficult problem of its longitude. Imagine a ship upon a meridian of the globe; suppose the earth's equator to be carried on to the heavens; it will then form upon the firmament what is called the celestial equator. If a line be drawn from the centre of the earth through the ship, on its surface, and be produced until it meets the firmament, the point of intersection will be zenithal to the ship. Now to determine the latitude of the ship, one simple observation is sufficient. Suppose the sun to be on the ship's meridian—or at noon, (and the time when this will occur may be easily found by observing the point at which its hight above the horizon ceases to increase. By an instrument well known to mariners the distance of the sun at noon in degrees from the zenith may be obtained.) Now there is in every almanac a column giving the sun's declination—or distance from the celestial equator—for every day in the year. Adding, then, this distance to that of the sun from the zenith, already found, we shall obviously have the length of the arc from the ship's zenith to the celestial equator. But this arc is concentric with the surface of the earth; and by knowing its length we know the number of degrees on a meridian between the ship and the equator, which is what we were seeking for, namely—its latitude.

But this is not sufficient to determine the ship's position; for it may be any where around the globe, so only the same distance from the equator be preserved. In order, then, to determine its position, its longitude must also be known, by which we mean its distance from the meridian of any given place on the globe. If we suppose a circle of the earth to pass through the pole and any given place as for instance New-York, and another through the pole and the place whose longitude we wish to find, the question really is what is the angle made at the pole by these two circles. This is theoretically more easily measured than the latitude; for the earth revolving on its axis through a circle of 360° in 24 hours, any place on its surface of course passes through 15° in a single hour. If then when it is 12 o'clock at New-York, we know that it is 1 at the place whose longitude we wish to find, we know that there must be a distance of 15° between the two meridians—or the longitude of the place is 15°; if when it is 12 o'clock at New-York, we know that it is 2 o'clock at the other given place, we of course know that the longitude is 30° &c. Thus if when it is 7 o'clock at New-York, we are quite sure that the sun is on the meridian of London, we know that the earth has moved through seven hours of time at the rate of 15° each hour; seven times 15 then will give us the longitude of the one place compared with the other. The difference of longitude then is always known, when we know what o'clock it is at the two places whose longitude we wish to determine. If it be one o'clock at Paris when it is two at Dublin, we know at once that the difference in longitude is 15°; if when it is one at Paris it be five o'clock at any other place, the difference of longitude is at once known to be 60°. These are astronomical elements taught at school, and therefore I take it for granted familiar to you all.

We see, then, that this problem, so famous for its difficulty, resolves itself to this: to find what o'clock it is at the same moment, both at the place whose longitude you wish to determine, and at that from which it is to be reckoned. As most books refer to London or Greenwich as the meridian from which the longitude is to be reckoned, I shall assume that as the starting point. Now suppose a ship at sea, far from land; the mariner wishes to determine his longitude, and, in order to do it, there are two things to be known—1st, what o'clock it is at his own point, and 2d, what is the time at Greenwich. Now, how are these two things to be discovered? It is always easy, by an accurate chronometer, to determine the first point, since the time may always be ascertained when the sun is on the meridian, and these observations may be repeated sufficiently often to keep the chro-

nometer accurate. If there be no chronometer at hand, various observations may be resorted to by which the time may be determined; so that the first of the two things necessary to be known may readily be ascertained. But the greatest difficulty is still behind, namely, to determine the time, at the same moment, at the meridian at Greenwich. You would say at first that nothing is more easy; for if, when the mariner leaves Greenwich, he takes with him a good chronometer, properly set, and if it be carried with suitable precaution against accident and disturbing influences, he may always tell the time at Greenwich. So he might, if so perfect a chronometer could be obtained, and if it neither gained nor lost any time on its passage. There would then be no difficulty in determining the time, and in solving the question of the longitude. But it is found that all chronometers are liable to accident, and that they will not go for any great length of time without varying so much as, in such delicate calculations, seriously to affect the result. To remedy this, two or three chronometers have been taken, so that, if one should fail, another might serve in its stead; and for a long while the British Government offered large rewards for the construction of chronometers so perfect as to determine accurately the longitude. But it at length became apparent that all such expedients however perfect they might be as works of art, were, after all, only expedients, and that the best of them was liable to failure.

At length astronomers bethought themselves whether a chronometer might not be obtained, made by the hands of Him who could not err, of a mechanism so exact that it should never gain or lose, a clock which never required to be wound up, and was not liable to accident. A clock was found, on the face of which *hands* were placed that could never err: that clock was the firmament—the hands, whose motions on its face only required the powers of the intellect to interpret them, are the Sun and Moon. All that is necessary is to put into the hands of navigators an instrument which shall enable them to determine accurately their positions.

In the common clock there is a dial, upon the face of which are cut certain fixed marks, called *hour marks*, which remain permanent and immoveable. Over these marks travel the hands and by observing their motions the time is readily known. Now suppose that instead of these regular marks upon the dial, there were spread other fixed marks, less regular in their arrangement and less easily understood than the marks of a common clock; but whose position may still be understood, and from which we can say that, if their position be this or that, the time will be thus or so. Now, upon the face of the firmament the stars are these fixed marks, and the Moon is the hand which continually changes its positions with regard to them. The relative positions of these stars are perfectly known, and every change of place among them of the Moon can be marked and distinctly understood. This is called the *Lunar Method* of determining the longitude, and at the present day it is the one in common use. Mariners are provided with a Nautical Almanac, in which are registered the various changes of the Moon's position; in that it is stated that when the Moon is in certain positions with respect to certain well known fixed stars, the time is so and so at Greenwich, and this is calculated and laid down for certain short intervals of time. When it is four o'clock at Greenwich, the Moon is so many degrees from a certain fixed star, &c. When the longitude is to be obtained, the distance of the Moon from several conspicuous fixed stars is first measured; the mariner then opens his almanac and finds that when the Moon has nearly the distance which he has measured, the time is such and such an hour at Greenwich. This once known, the calculation of the longitude is extremely simple, as I have already stated. In this way the position of a ship may be told within five miles. Place a vessel in the boundless ocean, far from land, with nothing but the heavens and the sea above and around it, and give the mariner a Nautical Almanac, and he can readily tell his position within, at the utmost, five miles of his true place. Some venture to mention a still greater degree of accuracy; and it is true that the latitude may be determined within a single mile. But there are difficulties attending the solution of the problem of the longitude which forbid this extreme accuracy. There is an interesting anecdote related by Capt. HALL illustrating the great accuracy of scientific calculation. He left San Blas, a port on the coast of Mexico, sailed south through the Pacific round Cape Horn and up through the South Atlantic, having been out of sight of land for eighty-nine days, intending his voyage for Rio Janeiro. About a week before the time when he expected to reach his port, he took what sailors call his *lunars;* and his calculations showed that he was within a very short distance of Rio. As the morning was foggy, he stopped his vessel, fearing that he might go ashore—and his hands were sent down to breakfast. By the time this was over the fog had cleared away, and right ahead in his course he saw Sugar-Loaf Hill, which bounds the harbor of Rio. The effect which such a power as this gives to the officers over their crews may readily be conjectured.

THE ELECTRIC TELEGRAPH.

Such are some of the gifts which Science has conferred upon Art. I will now mention one or two others; and one of the most recent is that of the Electric Telegraph, invented by WHEATSTON, and now employed in London. He had devoted to the subject of Electricity much time, and its first fruit was this discovery. Its object is, by the agency of Electricity to communicate between two distant places in a very short space of time. In England, it has already been applied along a railway, for some hundred or more miles. All that is necessary is to go to the office at one end and ask his question: in about three minutes he receives an answer. I chanced myself while engaged in railway operations, to witness an operation of this kind, and I shall not soon forget my astonishment at the result. I was standing near the office of the Great Western Railroad, when a passenger got out from the train of cars which had just arrived, over forty miles from London, went into the office, and told the clerk that he wished to send by the returning train a note to his hotel in London concerning a cloak and umbrella he had left there. "Yes, sir," said the clerk, "wait a moment and I will

give you the answer to your note." He turned to a small apparatus in a corner of the room, and in about three minutes told the man that his cloak and umbrella had been taken care of, and would be sent by the next train.

The method of the operation of this invention is extremely simple, and is easily rendered intelligible. I have already explained the nature of the galvanic current which is produced when zinc and copper, or other metals, are brought in contact. The electricity evolved at the surface of contact is decomposed, the positive taking one direction and the negative the other: hence if two wires be provided and put in contact, the one with the positive and the other with the negative fluid, these two currents will flow along them for any distance—even around the globe.—Now I explained to you at the last evening that if a magnetic needle be placed over the wire, along which this galvanic current flows, it will immediately be turned at right angles to the wire, no matter what was its former direction. Now suppose a wire be enclosed in glass tubes, or surrounded by some other non-conducting substance, and passed the whole distance from New-York to New Orleans; a needle placed above the wire at New Orleans will instantly turn at right angles to it upon sending along it the electric current.—This would be the case with any number of wires. Thus we have a method of communicating instantly between distant places. The next thing is to devise some means of rendering the communication intelligible. A variety of signals have been employed for this purpose. In Wheatston's telegraph the letters of the alphabet were employed, and the mode of conducting the communication was this: at each end of the route was provided a lozenge marked off into parallelograms, at the angles of which were placed the different letters of the alphabet. Magnetic needles were placed above the wires in such a position with respect to the figures that any two of the needles may be made to point to each of the letters by the action of the fluid, which, on being passed along the wires, causes them to turn at right angles to the wires. Now in communicating from end to end, the person who makes the first advance passes a current along a wire which lets go a bell at the other extremity; thus a wire touched at New Orleans would let go a bell here, and its language is, "I'm going to send you a message—so look about you." The person receiving the warning sends back a current, which lets go a bell at the other end, saying, in reply, "Go on, sir, I'm listening to you." Then they begin to spell out the words, if that is the method adopted. If E be the first letter, then two wires are touched, which will cause two needles at the other end to point to that letter on the lozenge, and so for all other letters. In general practice, fewer letters are necessary than those of the alphabet.

The manner in which these telegraphs are constructed in England is this: the wires, of which there are five or six, are placed in glass tubes, and buried several inches in the ground beside the rails. One is now nearly finished from London to Liverpool, and by it messages are transmitted with astonishing rapidity. For commercial purposes, these telegraphs are of especial importance.

QUARRYING STONES.

Another remarkable example of the contributions of Science to the Arts of life is derived from the properties of heat, as applied in the East to quarrying blocks of stone when the object is to excavate huge blocks from the surrounding mass. A groove is cut some two inches in depth in the required direction. This done, the groove is filled with fuel, which is kept lighted until the rock is highly heated. The rock, then, is of course expanded by the action of the heat; the fuel is then swept away, and cold water immediately poured into the groove. The sudden contraction causes the block instantly to split off.—The same principle is daily exhibited on our tables. If a heated glass be suddenly filled with cold water, it immediately breaks in pieces. In this way blocks eighty feet long and six thick are easily taken off with no other labor than that of chiseling out the groove.

A similar example of the application of Science to the economy of power is exhibited in France in the quarrying of mill stones. They are required, as you are well aware, to be circular and flat—cylinders with a very small altitude compared with the diameter—and the stone from which they are made is exceedingly hard. The mode of quarrying them is this: A very high, circular column of stone is wrought out, of the requisite diameter. To slice off portions of this, such as are required, by the common stone saw, would be a work of immense labor. A quite different agent is employed. At regular successive distances, grooves are cut around the column, into which are driven dry wooden wedges at evening. The dew which falls during the night, being absorbed by the wood, causes it to expand with a power so irresistible that all the stones are found properly cracked off in the morning.

BLEACHING, TANNING, &c.

Let us now turn to another science which furnishes aid to the process of bleaching. The discovery of chlorine and its bleaching properties altered at once the whole economy of this art.—The processes which before occupied weeks in their completion, are now performed with far greater efficiency in a few hours, by a mere application of a small quantity of some chlorides which destroy dark colors and immediately produce white.

There is a very remarkable case, which I must mention, where the simple application of a pneumatical principle has produced a vast change in a particular branch of art. In cotton manufactures, when spindles of cotton are to be put into the hands of the weaver, it is necessary that they be thoroughly wet—that every thread be saturated with water. The usual mode was to immerse them in a vessel of water, where they would have to remain for weeks before they would become properly wet. The air which fills the pores of the spindles effectually kept out the water which could only penetrate after a long time. The mode afterward employed is to expel the air in the first place; the spindles were placed in a vessel covered air-tight, which was put into the vacuum of the steam-engine. The consequence was that the air in the vessel rushed into the vacuum in which it was placed, and as soon as it had escaped, the air in the spindles having

upon it no atmospheric pressure forced its way out to fill its place. This was continued until the air in the spindles was completely discharged; when, by turning a cock, the water was let suddenly into the vessel, subjected to the atmospheric pressure, and thus forced into the vacated pores of the spindles. I happened, while on one occasion residing with a friend in the vicinity of a cotton manufactory, to visit it one day after dinner and to witness this operation. It struck me at once that the best way would be to fill the vessel in the first place with water, and then exhaust the air in it. The air in the water and in the spindles would of course be immediately discharged. This suggestion was carried out, and the method has been ever since adopted.

In the process of tanning, which consists in forcing certain liquid solutions into the pores of the leather, science has also provided a great improvement. The leather contains in its pores a certain quantity of air, which repels the water just as it did in the case just noticed. The leather was usually immersed in the liquid for several weeks, at the end of which it would become very well saturated. It occurred to some one that, by placing it in a vessel and exhausting the air above it, the air in the pores would make its escape, and its saturation become comparatively easy. This is the method now adopted, and an operation which formerly required months, is now performed in a few days.

The preservation of timber from what is called the *dry rot* depends on the same principle. The mode of preservation is by forcing a solution of corrosive sublimate into the pores of the wood; and in order to do this effectually, the same expedients are resorted to as in the cases already mentioned. This process has lately obtained to a great extent in England. It is called *Kyanization*, from its discoverer, Kyan; just as Galvanism and other scientific principles received their names from their discoveries.

We will now, if you please, change to other subjects.

THE FIXED STARS, NEBULÆ, ETC.

When I had the pleasure of addressing you at the last evening, I endeavored to explain the method by which we form an estimate of the distances, magnitude and splendor of the fixed stars nearest to our System. We saw the conclusions of astronomy drawn from a due examination of the circumstances by which these distances are governed. Now the number of stars of the first magnitude to be seen in the heavens is extremely small—not more than fifteen or twenty being visible. Outside of this class the stars of the second magnitude exist in greater numbers, there being from fifty to sixty known; while there are of the third magnitude more than two hundred to be seen. I should observe that we are not to suppose that there is no difference between stars of the same magnitude in point of size and splendor. Those of the first magnitude are by no means all equal, as Sirius, for example, is much brighter than the others. Nor must it be understood that there is any definite line of distinction between stars of different magnitudes. The form of speaking is just as if I should speak of men of the first magnitude, meaning those above six feet high; those of the second between six feet and five feet six, &c. This would be a perfectly arbitrary distinction, and so is that of the stars. Now astronomers have seen the necessity of some classification of this sort, and have, therefore, divided the visible stars into seven orders or magnitudes—the smallest being of the seventh. As to the number of the stars, it cannot be told with any approach to accuracy. When we get up to those of the fifth, sixth, and seventh magnitudes, their number is beyond computation. The probable number which may be distinctly seen by the naked eye has been estimated at 20,000; but this must be taken as a rough estimate. When, moreover, we come to apply the telescope to the examination, we discover a farther series of stars of the eighth, ninth, tenth, &c, to the sixteenth and seventeeth magnitudes, which are far too small to be visible to the naked eye, or which appear only as a whitish cloud, but which the telescope tells us are composed of an infinite number of stars, as it were, powdered over the surface of the firmament.

If, now, the nearest fixed star—that one which can be seen under the most favorable circumstances—be separated from our system by so vast a chasm as we found at the last evening, what must we suppose to be the case with those of the sixth, seventh, and still more of the sixteenth magnitude—visible only by the most powerful telescopes ever constructed? But computations have been made which still farther excite our wonder and astonishment. A star of the seventh magnitude can easily be compared with one of the first, in point of splendor, by the photometer—just as the light of a sperm candle can be compared with that of a lamp. Sir JOHN HERSCHELL has compared the splendor of a star of the sixteenth magnitude with that of one of the first, and has found that the light of the latter is equal to three hundred and sixty-two times that of the former.—From this it may be inferred that the distance of a star of the sixteenth magnitude is such that it would require thousands of years for its light to reach our system. These considerations present to our minds most comprehensive views of the economy of the Universe. For if light requires a thousand years to come from any of these plainly distinguishable stars, there can be no doubt that it takes twenty times as long to come from others; and what are we to infer from this but that there are visible objects in the universe which 20,000 years ago existed as they are now seen. Light left these stars 20,000 years ago, and has just reached the earth upon which we live. For 20,000 years past, then, these stars, for aught we know, may not have existed. The objects we see to-day are not the objects of to-day; the Sirius that we see to-day is not the Sirius of to-day. The light by which we see it left that star three years ago, and from that day to this we have known nothing of it. Into what a singular historical state does this view throw creation! Our system, then, exists at an enormous distance from the nearest of the fixed stars, and look in what direction we may, the same chasm yawns between us and it.

The telescope of Sir WILLIAM HERSCHELL was of a power that has never been possessed by any other, and many curious results have flown from

his survey of the heavens. If we cast our eyes upon the different portions of the firmament, we find the stars very unequally distributed, in some parts being very closely clustered, and in others scattered at great distances from each other.—There is a line across the heavens called the *Via Lactea*, or Milky Way, presenting the appearance of a mass of whitish light—traversing the whole arch of the firmament, composed of stars so thickly clustered together as to appear blended in one mass of white light. Now after the most close and careful investigation, Sir William Herschell came to this conclusion: that our Sun is one of a vast stratum or bed of Suns, the length of which is very great compared with its breadth, and its breadth great when compared with its thickness. [To assist the imagination in forming a conception of this stupendous idea, Dr. Lardner exhibited a copy of a drawing made by Herschell to illustrate his theory.] The appearance of this stratum is that of an oblong bed of stars, in which they are crowded as closely together as possible. (It must be borne in mind that these words are used with reference to a set of notions entirely different from those with which we are familiar.) Now Herschell accounted for the apparent difference in the distribution of stars through space in this way: If a person placed on the earth sees the stratum through its smallest diameter, he looks directly through it, and the stars appear at some distance from each other; if he sees it through its breadth, he sees them closer together; and if he looks through its length, they appear denser still. In this latter case they have the appearance of a white light; and Herschell supposed the Milky Way to be a stratum seen through its length. These views are borne out by appearances so strongly corroborative of them that the philosophical world has generally given to them its assent.

And now I beg you to endeavor to strain your imaginations—for the mind *aches* in the attempt to grasp views like these—to a full conception of these stupendous discoveries. Remember it has been proved that the distance of the Sun from the nearest star of this immense bed is such that light can traverse it only in three years, at the amazing velocity of 200,000 miles in a single second. Nothing is more certain than that every star is at least as far as this from the one nearest to it. I spoke of the stars as clustered closely together; but I said this with reference to dimensions of which this whole mass of stars is but a point.

With regard to the double stars, I shall have some things to say calculated to raise in the mind conceptions which I think will be fruitful of good. On a careful examination of some of the double stars, Sir William Herschell thought it probable that they were two suns at an immense distance from each other; their apparent proximity he accounted for by supposing that we see one behind the other, the view being thus fore-shortened. If so, he reasoned, it will be well to ascertain the effect of the earth's motion in its orbit upon their apparent distance from each other. The immense diameter of this orbit he supposed would of course produce a change in their apparent distances. With reference to this he set about his examination; and the result was one of the most important discoveries in modern astronomy—one of the numerous instances which continually occur in which a person, seeking what is not to be found, finds that for which he is not seeking and which he did not expect to find. He watched these stars, and found that they did change their relative position. This inspired the hope that at last he had obtained the long sought effect of the earth's motion in its orbit on the motion of the fixed stars. But he soon saw that the change was quite different from the one he had expected, and his first feeling was one of disappointment. But a sentiment the reverse of this soon succeeded it. He found that these two stars were in motion with respect to each other, and on pursuing the inquiry still farther, he arrived at a fact of absorbing interest—that those twin stars were members of a double Solar System, and in motion round each other in paths similar to those marked out by the planets; their orbits he found were elliptical. Thus he found that they obey the same law of gravitation that Newton discovered, and arrived at one of the most magnificent conclusions in the whole range of science, that our Solar System exerts an influence upon the remotest parts of the Universe. He saw in the fixed stars the same mechanism which is traceable in the arrangements of the Solar System; one of the many evidences brought to light out of the store house of Philosophy of the greatness and power of the Creation.

LECTURE IV.

POPULAR FALLACIES.

LADIES AND GENTLEMEN: I observed on the last evening that Providence, in supplying us with the means of intellectual food, has marked its provision by the same conditions as those which prescribe the labor of our bodies—namely, that they are only to be acquired by our individual industry. Now, by pursuing the same course with reference to this fact as to the other, we shall find that the same economy is observable through all the arrangements of God, in interpreting the phenomena of nature. When a question arises as to any matter of fact or appearance, what is more common than the observation—Must I not believe the evidence of my senses?—And yet there is no evidence so fallacious, nor do any witnesses require such strict cross-examination as these very senses. It is true that in some particular applications of them the testimony they give must pass, as testimony ordinarily passes in our courts of justice—for what it appears to declare. But in any case of doubt, requiring particular examination, the senses are of all the most fallible witnesses. I propose, in commencing this evening, to call attention to a few examples of the very fallacies of which these senses are continually guilty; and the most striking, and those which in most minds produce the greatest incredulity, are the appearances of the heavenly bodies when they rise and set. You are all familiar with the rising and setting of the Sun. You all know that the impression produced on the mind is that of an extremely large orb—an object prodigiously larger than the appearance the same Sun presents when it is in the more elevated portions of the firmament. Now there is nothing more certain

than that the real magnitude which the Sun and Moon presents to the eye when near the horizon, is, not to say not greater—but, if you push me to the strict limits of truth, is in fact apparently less than when they are higher in the firmament. I am quite prepared to meet the exclamation, which has been made a thousand times before—'What! tell me that I am not to believe the evidence of my own eyes? Do you tell me that I do not see that the Moon is larger when it is nearer the horizon than when higher in the heavens?' I *do* tell you so—and I think that I can soon convince you of it, or at least enable you to convince yourselves, which is still better.

Now in the first place, if you please, I will put before you the evidence of the fact that these visible objects do not appear to the eye larger when just at the horizon than after they have risen higher in the firmanent. After you are satisfied of this—rationally and intellectually convinced—it will remain behind to account for the impression produced on the mind, for there can be no doubt that this is larger in the former case than in the latter. Now the way in which it can be shown that this is not the fact is very simple. Astronomical instruments are furnished with means for measuring the appearance which celestial objects have to the eye. At present I will only state what the means are, as I shall hereafter have occasion to speak of them more minutely. When you look through the telescope you see a visible circle called the *field of views*—the portion of the heavens which the telescope enables us to see. Now, in an astronomical telescope, the intention of which is not merely to gratify curiosity, by showing celestial bodies more plainly to the observer, there are certain means provided to measure the apparent size of the heavenly bodies. There are certain wires or hairs extended across the field of view, and appearing like fine, black, parallel lines; these are moveable by screws which carry them up and down parallel to each other. By this screw, which is called the *micrometer screw*, and of which I shall hereafter have more to say, when we bring into the field of view the Sun or Moon, by raising one of the wires, so that it shall just touch the upper edge, and the latter so that it may just touch the lower edge of the object, we have in the interval between the actual apparent width of the body to which our attention is directed. Now suppose this has been done when the Moon is rising: I shall then have its actual width as it appears to my eye. Let the wires, then, be left undisturbed in this position, and wait until the same object shall have reached the upper parts of the firmament, when it is generally believed to appear smaller than before. Then bring the telescope to bear upon it again; and it will be found that the same wires touch the top and bottom of the image precisely as in the former case. Now this experiment will be sufficient to convince any one that as a matter of fact there is no difference in the apparent size of the object seen in its different positions.

But, you may say, the evidence of the impression is so strong that I cannot help thinking that there is some influence in the telescope which vitiates the real appearance of the objects; and I cannot give up the impression that the same object appears larger at the one time than at the other. Give up the telescope, then, and try the same experiment in this way: Take two threads of silk—(and any lady who chooses can easily do this)—place them parallel to each other, and bring them into such a position that they shall be, the one between the eye and top of the Sun, and the other between the eye and the bottom of the Sun. Keep them thus as well as you can until the Sun has advanced to a higher point in the sky; then bring them again to bear, and you will find that the threads, at the same distance as before, will measure the width of the Sun. This must certainly satisfy all reflecting minds that this appearance of greater size near the horizon is a deception—I will not say an optical deception, for I have shown that the eye is not deceived—the visual angle in both cases being precisely the same. The question—what causes this singular appearance? has been for a long while one of no little difficulty. Various solutions have been proposed, and the one generally adopted by scientific minds I will now endeavor to make plain though I fear its nature is so remarkable that I am not sure I shall make it intelligible. But here it is. If the Sun or another celestial object be near the horizon and I direct my attention to it, I see between me and that object a vast number of objects upon the face of the earth—as trees, houses, mountains, &c, the magnitudes and positions of which are familiar to me. These supply the mind with a means of estimating the size of the object at which I am looking. I know that it is much farther off than these; and yet the Sun appears, perhaps, much larger than the top of the intervening mountain. I thus compare the Sun by a process of the mind so subtle and instinctive that I am unconscious of it, with the objects which I see between it and myself, and I conclude that it is much larger than these. Well, the same Sun rises to the meridian; then there are no intervening objects whereby to space off the distance, as it were, and thus form a comparative estimate of its size. The same thing may be observed if we look upon the firmament on a starry night; it will appear like a flattened vault, the vertex being nearer to the earth than the horizon. This is nothing more than a repetition of the same cause. Mark the position in which the mind is placed. You see the same Sun at its meridian, at a distance which you think is less; the same Sun is of the same size; but the mind reasons that inasmuch as the distance is less in the latter case, therefore the Sun must appear less. I am prepared to be met by the objection that this is an extremely learned and metaphysical reason. So it is: the operations of the mind are extremely subtle, and if we want an explanation of them, we must be content with one drawn from their essential nature. As well might we complain of the solution of a problem in mathematics, because it is mathematical, as of this explanation because it is metaphysical. One fact is most certain—that neither the Sun and Moon, nor their appearances, are larger when near the horizon than when in the higher regions of the firmament. This is perhaps, in the whole range of science, the most striking of the fallacies of which the senses are continually guilty, and which the operations of the mind are required perpetually to correct.

Another not uncommon fallacy of the senses re-

fers to the conclusions at which we arrive with reference to the sense of touch. When we touch any object we are sensible of cold or warmth; and we are accustomed to say that this or that is warm or cold according as we feel it. Nothing can be more in discordance with fact than the results of such evidence. Suppose yourself in a room where there is no fire, and at this season of the year. Touch with your hands any metallic object—the fire-irons for instance; you immediately become impressed with the conviction that the iron is cold. Touch then the cloth upon the table—and you believe it warmer than the iron. Touch the bare table or the floor, and you believe it less cold than the iron and less warm than the cloth. Now if you could imbed a thermometer in iron, the table and the cloth, you would find that this instrument would tell a far different tale from the touch. It would tell you that there was precisely the same temperature for them all. It is a physical truth that the three bodies are of exactly the same temperature. Now, you may say, of what use is the touch if it deceives us in this most egregious manner? It is of much use. The same Providence that has supplied us with this instrument for certain important purposes, has also, for the same end, supplied an intellect to correct its errors.

Hear now the reasons why these objects seem to the sense of touch, of different temperature, while they are all of the same.

The human body has a certain fixed temperature, generally higher than that of the temperature to which it is usually exposed, certainly higher than would be found in any room in which was no fire, at this season of the year. All bodies, in reference to that, have certain properties which, in order to clear up the matter, it may be necessary to explain in a few words. They have certain powers of conduction which are different in different bodies. If two bodies of different temperature be placed in contact, the heat will be conducted from the one to the other, with a different degree of facility in different bodies, until both be of the same temperature. Suppose the hand, of a temperature by the thermometer of 100°, be put in contact with a piece of iron whose temperature is but 60°. The iron, then, being a good conducter of heat, would rob the hand of its superior temperature, which will lose the heat given to the iron with a rapidity proportioned to its conducting power. Now, suppose the hand to be placed upon a piece of wood at the same temperature, which is not so good a conductor, and yet has a certain conducting power: the heat of the hand will not be lost so fast. Finally, place the hand upon the blanket, which is likewise at the temperature of 60°; the heat is now scarcely lost at all. Nay, after keeping it upon the blanket for a little time, it will cease to lose any. Now, what does this amount to, in explaining why the iron is colder than the wood and the wood colder than the cloth? Simply this: neither of these objects is colder than the other, but one has the power of conducting the heat from the hand more rapidly than another; and the sense of cold is produced, not by any real degree of cold, but by the conducting power carrying the heat away from the hand—and its loss giving the sensation of cold. You see how easy then, by the application of one or two scientific principles the fallacy of this sense is explained.

This leads me to another observation respecting expedients to which we all resort with reference to heat and cold. The whole theory of clothing rests upon the facts and principles to which I have just adverted. The human body is by some inscrutable arrangement supplied with an internal fountain of heat, by which its temperature is maintained over the air which usually surrounds it. The fountain of heat owes its origin to the same unknown principle as organization itself. The fact of its existence, and that it is capable of supplying a certain quantity of the calorific principle, are all that we can know. This amount of heat is essential to the well-being, health and comfort of the human body: if we lose it too fast the sensation of cold is produced: if it is not suffered to pass away at all, we become sensible of fever. The atmosphere is subject to various vicissitudes of heat and cold, and from both man is compelled to defend himself. How shall he do it? He surrounds himself with clothing, made of non-conducting materials, such as flannels, cotton, &c.—The effect of this is to keep the body at its natural temperature by placing a barrier between it and the atmosphere—not allowing the heat of the body to pass off from the surface. If the external air on the other hand be hot, the same covering which prevents the natural heat from passing away, hinders the external heat from entering. Hence in hot climates as well as in cold persons are accustomed to wear flannel clothing, to produce opposite effects in the two cases.

Another point in the theory of clothing is deserving of notice. Nothing is more conducive to the regular temperature of the body than free evaporation. The surface of the body is an admirable piece of mechanism. The skin is an instrument to which few present have given the attention it deserves. It is filled with minute capillary tubes, which continually send forth the effluvia which ought to escape. If you cover the body with a substance which is a non-conductor not only of heat but also of air, in seeking to preserve the regular temperature of the body you obstruct the escape of those principles which ought to pass freely away. Hence flannel is mostly used for clothing, as its pores are open and the air readily passes, while heat only makes its escape through it with reluctance and difficulty. When the cloths which have been called *Mackintoshes*, from the name of their discoverer, first came into use, it seemed that people never could have enough of them. Dresses of every kind, but especially shooting-dresses, boots, trowsers, and all, were made of the Mackintosh cloth. They were impervious to heat and likewise to all fluids. Consequently all exhalations from the body were stopped by these coverings, and serious injuries to health, including many lives, were the result of their use. People soon began to understand their character, and now this cloth is rarely used in England except for cloaks, which, being open, allow space for the evaporation of those noxious fluids. In alluding to the functions of the skin, there is one circumstance not uninstructive. You are aware that when we inspire, the air taken into the lungs undergoes a change, with the nature of which we are not acquainted; this we

know, that when it is expired, its nature is changed and it has acquired the qualities of carbonic acid gas. We find that this result also illustrates the functions of the skin. If you take a wine-glass or tumbler, and inverting it on the skin, hold it firmly for a short time, the air in the glass will be found to have undergone a change analogous to that which is breathed from the lungs: a candle thrust into it will immediately be extinguished. From observations of this kind it is evident that the whole surface of the body is, as it were, an extension of the lungs, dealing with the atmospheric air in the same manner with them, though with considerable less energy.—Whatever change the lungs effect on the oxygen of the air brought into contact with them, is effected by the skin in a feebler degree on all that gets access to it. Now to surround this with an air-tight dress, as if caoutchouc or similar fabrics, would be the same in principle as to stop the mouth and obstruct the respiration, with reference to the function of the lungs.

I will give one farther example of the fallacy which gave rise to these observations. The water of a cold bath is no colder than the surrounding air; yet such does not appear to be the case to a man about to plunge into it. Yet if a thermometer be first suspended in the air, and then plunged into the water, the same temperature will be exhibited in both cases. What, then, makes the difference in the sensations? This is easily explicable upon the principles I have been considering. It is true that liquids are not good conductors of heat in the strict sense of the term; they operate in a different way. If a person plunge into water the temperature of which is lower than that of the air, the particles which first touch his body become warmer, and, by a well known law, rise immediately to the surface. A fresh stream of cold particles rushes to his body, which in their turn rise and give place to others. Thus there is kept up a constant stream of cold particles towards his body, which clearly accounts for the apparent temperature. Air may be heated to the temperature of boiling water, and still a person may remain in it without inconvenience—when, if plunged into water of the same temperature, he would be immediately scalded. These observations clearly account for the exhibition of a feat in England, and, for aught I know, in this country, of one M. Chabert, famous for exposing himself to a degree of heat which it was thought would destroy any other person. He had an oven, which he was accustomed to have heated to such a degree that he could broil a beef steak by its heat, while he quietly sat in the oven and cooked and ate his steak. But Chabert did not stand upon the bottom of the oven; he took care to have between his feet and the iron which formed its bottom, and on which the steak was cooked, some non-conducting substance. Air not being a good conductor, and by being careful to have a carpet or piece of cork upon which to stand, he could quietly remain in the oven without injury. So that these feats were nothing more than an experiment on the principle of conductors and non-conductors. You see, then, that all these extraordinary performances are accounted for by the simplest scientific principles.

The same man pretended to swallow Prussic Acid. Now there are certain liquids which no one ever swallowed without swallowing a portion of this acid, which may be taken in very small quantities with impunity. Chabert's feats in London were stopped very suddenly by a gentleman who brought to the concert a bottle of real Prussic Acid, and proposed to the juggler that he should swallow it, which, however, he very prudently declined to do.

The principle of the non-conduction of heat is illustrated by the economy of steam-boilers in England, where it is a great object to produce as much steam as possible with the least possible expense of fuel. In Cornwall nothing is more striking than, in hot weather, to walk into the boiler room and find it the coolest place to be found. The boiler in this case is surrounded by the best non-conductor to be found; and the material which is found to answer the purpose is saw-dust. The Cornish boilers, which are used at a temperature as high as those of your high-pressure engines, are coated with three or four inches of saw-dust. This non-conductor prevents the escape of the steam, and all that is evolved by combustion is kept within the boiler, and the room is kept as cool as any one need to be. I have alluded to the fact that flannel is frequently used in warm climates as a defence against the heat. Now blankets were frequently, upon the same principle, put around steam-boilers, before the superiority of saw-dust was known. Ice, which I understand is conveyed from Boston to India, is also wrapped in blankets; this is nothing more than an illustration of the principle I have endeavored to develop.

LIGHT.

All the impressions of the senses require to be interpreted by the understanding, and this brings us back to the truth that the Creator, who has given us the intellect, intended that we should use and apply it to all the phenomena of the senses, which give such fallacious testimony.—We are accustomed to rely on the evidence of the eye with the most implicit confidence: and yet there is no one of the senses whose testimony is so fallacious as that of this same eye, all-perfect as it is in mechanism. The impressions made upon the eye, when carefully and philosophically examined, are extremely curious. With the structure and arrangement of this organ you are all doubtless familiar. Light is not a material substance; it was, however, long supposed, and is still held by many that this is its nature; but according to modern discoveries, it is only caused by the undulations or vibrations of the medium through which it passes. The best way, perhaps, of elucidating this point is from the analogy of sound, as the same laws which govern the one govern the other also. No one supposes that sound is a material substance; you are all aware that it is merely the effect of a system of pulsations produced in the atmosphere and propagated through that form the body struck to the ear. Now if you only admit the existence in nature of a fluid, infinitely lighter, more attenuated, elastic, and delicate than the air, there will be no difficulty in conceiving it to be a saystem of pulsations transmitted through the fluid, infinitely more delicate and rapid than those of sound. This illustration conveys the best no-

tion I am capable of giving, of the modern theory of light.

The sun or a lamp acting on this æther, as it is called, puts it into a state of pulsation; the vibrations passing through it as those of sound through the air. This pulsation is propagated to the eye—reaches the retina, and puts that delicate membrane into a state of tremulous motion which is the proximate cause of the impression of light produced in the mind. It has been, moreover, discovered by modern science that the varying rapidity of these vibrations is the cause of the difference in the colors of the spectrum; and what is still more remarkable, these vibrations have been subjected to admeasurement. The various colors, blue, green, &c, are nothing more than the effects of the different rates of pulsation imparted to the retina at the back of the eye-ball. A ray of vibration enters the chamber of the eye through the pupil—a small blackspot in the centre of every eye, which is merely an aperture through which a rod might be thrust—and causes the retina to vibrate at different rates. Science has discovered a method of computing the rate at which this membrane pulsates; and the number of vibrations per second, when the sensation of redness is produced, and so for the other colors. There would be nothing extraordinary in this if this was any ordinary rate, as for instance fifty times in a second. But when I tell you that the number of vibrations for one color is six hundred millions per second, seven hundred millions each second for another, and that it is never less than six nor more than nine hundred millions—when I tell you that modern science has estimated this with close accuracy, you will admit that it has accomplished what approaches very near to the miraculous.

These observations have been suggested by reference to the fallacies into which we are led by the senses—and this is especially applicable to the impressions of the different colors; for the truth is, that probably no two persons receive precisely the same impression from the same color. There are numberless instances of different impressions made upon different individuals, and nothing is more common than an inability to distinguish between green and blue. There are hundreds of persons who are never able to distinguish by their colors the cherries upon a tree from its leaves. The celebrated DUGALD STEWART, the well-known chemist DALTON, and many other names, probably familiar to you all, might be mentioned of persons who were unable to distinguish the different colors.

By a little management we may be able to see bodies that do not exist, and if we take the evidence of the senses on these points, we should be led to believe in all sorts of spectres—the effect of factitious vibrations produced by various causes. It would require weeks to enumerate all the exhibitions of this deception; but I will mention one or two which may amuse, and at the same time be instructive. Take a stick of red sealing wax and place it between the eye and a sheet of white paper; after keeping the eye steadily fixed upon the wax for a short time, look beside it, and you will see a stick of blue wax as distinctly as you perceive the real wax. In this way a succession of spectra may be produced. Thus, by looking steadily at a red wafer for a short time, you will be able to see beside it the *ghost* of a blue wafer; and conversely a blue wafer will give birth to the ghost of a red one—these two colors being correlative to each other; the retina, by the action of the one, is put into a state of morbid vibration by which the effect is produced. It is explicable by supposing that when the retina is put into a state of pulsation, its motions continue for a short time, just as a bell continues to ring for some seconds after it is struck. There are some instructive and amusing toys constructed on this principle, which it may be well to mention. There is a certain toy which exhibits this:—around a circle of card, having their feet in such positions as would be successively assumed by two persons waltzing, are two figures so arranged that when one position is presented by turning the card around a little the next is exhibited. After passing completely round, the first position is again shown, the figures having made a complete revolution. Now, if looking through a small aperture, a pin hole through a piece of paper, so that one attitude of the figures can be seen at a time, and the toy be then turned swiftly about, to your eye will be exhibited the appearance of two persons waltzing, just as they would be in the real waltz. The explanation is simple: when you look at the first position, the vibrations caused by the retina are continued for the extremely small position of time until the next position comes into view, and so for all the rest; so that the impression produced is that of a connected series of positions resembling those of the waltz. That this is the true explanation is made evident by the fact that if the card be turned too swiftly, nothing but a confused impression is produced; and if too slowly, there will be an impression of an irregular motion.

It is mentioned in the Programme of this lecture, that the resemblance between the natural organs and artificial instruments would be explained, but I have only time to allude briefly to that of the eye and telescope. The eye, as you know, is a compound lens: when a visible object is placed before it, its image is produced on the hinder part of the interior chamber of the eye.—Now a similar contrivance is employed in the telescope. The object-glass in this instrument corresponds to the crystalline lens in the eye, made of glass, and in the double convex form. In the eye, when an object is placed before it, the rays of light are conveyed to a focus, when a small image is produced; and the same thing is accomplished in the telescope. If you imagine that when you view an object through the telescope you see the real object, you are greatly mistaken; it is not the object itself, but only a picture of it, in such a position as to enable you to apply to it a microscope. This picture is but one or two inches from the eye; and you are to deal with it, in applying the microscope just as you would with a bit of animal fibre, or any other small object that you wish to examine. Thus the telescope is nothing more than a device for obtaining a small picture, near at hand, of a distant object, so that the powers of the microscope may be applied to it. The microscope is nothing but the eye glass near which is the picture. Thus it is not the object itself, but only a magnified picture of it, which is seen by the telescope. With these observations I conclude this branch of the lecture.

TEMPORARY STARS, &c.

Among the extraordinary phenomena which the heavens from time to time present, there are none more inscrutable—none which surround even astronomers with more wonder and perplexity, than the appearance of TEMPORARY STARS. Indeed the views which such appearances open to the mind, of the economy of the universe, are most extraordinary. From time to time, phenomena like this have been exhibited; a star makes its appearance in a point of the heavens where none has ever been seen before, and of a magnitude and splendor far superior to those of the stars by which it is surrounded. It continues visible for a time, gradually declines in splendor, finally disappears, and is never seen again. In the year 389 there was an appearance of this kind, but in 1572, there was one much more remarkable. A star appeared so suddenly, that in half an hour from its first becoming visible it had reached a size equal to that of a star of the first magnitude. An astronomer of that day was observing the same portions of the heavens and saw nothing remarkable—nothing which was not perfectly familiar. He retired to his dwelling, and in a few moments some peasants ran in and astonished him by saying that a star had blazed out and suddenly assumed the splendor of a star of the first magnitude. He went out and found that it was so; it exceeded Jupiter in splendor, and was for a long while so bright that it could be seen in the day, in the immediate presence of the sun. It continued thus for about two years, when it disappeared, and has never since been visible. There are several instances, not quite so remarkable as this, which have occurred in a similar way. What purposes these stars subserve in heaven's high economy—whether they are suns called into being by the Maker of the Universe, and permitted for a time to exist until they shall have fulfilled the purposes of His wisdom is only a matter of conjecture. The fact of their appearance is unquestionable; the witnesses have been scientific men, and their number is so great as to preclude all doubt. Beyond this we are entirely ignorant.—What I have told you, Science tells, and no more. The facts are all before you.

The appearance of periodical stars is also very extraordinary. They are subject to great variation in their apparent splendor—being at first of small magnitude: gradually, after the lapse of time, increasing in splendor to that of a star sometimes of the first, and at others of the second magnitude; then they decline in size, and soon become invisible. But by watching the spot where they were first seen, after a lapse of time, they will be seen to reappear, increase in brightness, then decline and disappear for another period. Their recurrence is observed to be periodical. Of course, when a phenomenon of this kind is observed to occur at regular intervals, a variety of theories will be suggested to account for it; and astronomers accordingly have devised several hypotheses whereby to explain it. One of the most plausible is that derived from the existence of spots upon the sun's surface; and it may easily be conceived that one side of a distant sun should be entirely covered with spots; so that when that side is presented toward us, no light proceeds frem it; whereas, when the other hemisphere, which may be unobscured, is toward the earth, the full splendor of the star is apparent. It is observable that there are periodical stars which never become quite extinguished—varying from the second to the sixth magnitude, at regular periods. If we suppose, then, that the surface of distant suns are covered with permanent spots, there will be no difficulty in imagining that, as the body turns upon its axis, its appearance to us would be precisely that presented by the periodical stars.

FALLING STARS.

There is another subject that I might have mentioned in connection with what I had to say of aerolites; but as I am not tenacious of method in these Lectures, so useful and important information may be communicated, I shall say something of it here. You are familiar with the phrase, *falling stars;* the phrase used by the French philosophers to designate these phenomena is *étoiles courantes*, of which ours is no doubt a corruption. The name, however, is of but little consequence. You are all familiar with the appearance of a stream of light which seems to rush across the heavens, with an appearance like a meteor. Now, nineteen persons out of twenty would ascribe these phenomena to electrical light; but this is a mistake. The *étoiles courantes* present appearances which, on careful investigation, prove that they are not asmospheric, but cosmical phenomena; that is, that they do not belong exclusively to the earth, but to the solar system. By observations taken from different points on the earth, it has been ascertained, by the same principles by which we found the distance of the moon, that these *étoiles courantes* are greatly beyond the atmosphere, at a hight of almost six hundred miles, which is much farther than our atmosphere is known to reach. There are many causes, arising from the more advantageous circumstances under which they may be seen, which should render them objects of extreme interest in this country. They generally make their appearance twice during each revolution of the earth in its orbit—about the 11th of November and near the middle of July. They often appear in a shower, as it were; and it has been supposed by many that they are bodies revolving around the Sun until encountered by the earth—too small to be visible except under very peculiar circumstances. It is evident that they belong to the general class of aerolites; their magnitude is often six or eight hundred thousand tons, and, from fragments which have been examined, their consistency is known to be as great as that of granite. To explain, as well as can be done, their origin, would be merely a repetition of what I said concerning aerolites. The question may arise—Whence comes their light, since they are non-luminous? This is easily explained, upon the principle to which I have often alluded, that bodies may move through the air with such a velocity as to evolve both light and heat.

I have already alluded to the fact that when we examine the stars of the firmament we find that many of them are not single stars, but double; that they are connected together by a physical tie, twin systems revolving about each other. The difference in their colors, and their always preserving the relation of the harmonic colors, though worthy of notice, are facts not capable of

satisfactory explanation. This may belong to the class of deceptions which I have already noticed this evening. Although no one supposes that they are not double stars; still though one is red the other may not be blue, as it is in appearance; the eye may be so impressed with the red as to see its correlative blue, just as in the case of the wafer.

The number of double stars is very great—several thousands being distinctly visible over the firmament. The triple stars have motions, relative to each other, as mentioned last evening. They move in ellipses, in obedience to the law of gravitation. This, as established by NEWTON, declares that if two bodies have a mutual attraction they will always move the one around the other in ellipses. Sir William Herschell found that the double stars have just such motions as are prescribed by this law. There can be no doubt, then, that the sway of the principle of gravitation extends from the solar system to such distances as to leave no doubt that it is part of the system of the Universe.

In looking upon the heavens we are struck with the appearance of clusters or groups of stars, called Constellations or the Great Bear, Orion and others. It is most likely that this particular arrangement is entirely casual—there being no necessary physical connection between the seven stars of the Great Bear which should prescribe this especial arrangement. But, when we examine them with a telescope, we find other groups to which these observations will not apply. Some of these appear like clusters to the eye, while others are merely white spots; they are of various forms, generally of a spherical or globular shape. Some of them appear as distant stars, which may be counted; while others have the appearance of a whitish cloud, even to the most powerful telescopes. There are also collections of stars to be seen analagous to the bed of stars which I explained last evening, of which our sun is an individual member. When we look out upon the farthest bounds the mind can ken, we find minute objects which even to the telescope have the appearance of stars powdered over the firmament. In other cases, we may doubt whether we can discern the individual stars or not—some maintaining that they can, while to others they have the appearance of dim, cloudy spots. That all these are composed of individual stars, analogy leaves no room for doubt.

In many portions of the firmament, as you see by this diorama, are presented appearances of faint streaks of light: there is reason to believe that these also are vast beds of stars, which present themselves to our view edgewise, as I explained at the last evening.

Many of these nebulæ are annular, composed of stars placed in the form of rings, which we see in all directions, some directly in front, and others at every possible angle of obliquity. There is one, which is here exhibited, which has the form of an hour-glass or dumb-bell. I might observe that when a star is observed through a telescope, it ceases to scintillate; so that the appearance on the drawing is more accurate than might at first be supposed.

The number of nebulæ catalogued and distinctly recognized by astronomers amounts to between three and four thousand. The observers who have devoted most attention to the subject have estimated the distance of some of these nebulæ to be so great that it would require 30,000 years for light from them to reach our earth. It must be, then, that they have continued to exist for that length of time, and, for aught we know, may have been blotted from being for so many years past.

LECTURE V.

HISTORICAL SKETCH OF ASTRONOMY.

Dr. LARDNER commenced his Lecture on Monday evening by exhibiting experiments to illustrate the tendency which nearly all bodies have, in revolving about an axis, to turn upon what are called their *axes of preference*. These experiments have already been described in our report of a former Lecture, and it is not here necessary to repeat them. There were one or two other principles illustrated in these last experiments not hinted at in the former, which it may be well to notice. One of these was shown by suspending a flexible ring—represented by a looped chain—and put into rapid rotation around its longest axis. The chain first spread itself out into a ring, and then assumed a position which allowed it to turn about an imaginary axis perpendicular to its first. Another fact exhibited was, that, in causing a cylinder to spin about its longest axis, when it came to assume a position which enabled it to turn upon the short axis perpendicular to the other, it presented the appearance of a small sphere surrounded by an apparent atmosphere: this is easily explained, since, in revolving, the central portion of the cylinder is immediately and continually under the eye, while the other parts are continually shifting their position. These experiments, and others similar to them, are very simple and easily made; and the Doctor said that he could not too strongly recommend, at this holyday season, when it is usual to give presents to children, that, in selecting gifts, those which shall be instructive as well as amusing be chosen; as, for instance, kaleidoscopes and many simple instruments easily made and of an interesting character. There are hundreds of ingenious men among us, he said, who could readily construct them. After these remarks and experiments, Dr. LARDNER proceeded with his Lecture as follows:

LADIES AND GENTLEMEN: I purpose to call your attention, on this occasion, to a subject which will prove extremely interesting, if I can only make the spirit of it intelligible; although I know that it is extremely difficult to do this to any considerable extent, I do not wholly despair. The Creator of the Universe, ladies and gentlemen, has written two books, in which we are enabled, by the faculties he has given us, to read his attributes and his laws. And these, like all the productions of he same author, have a most marvellous similarity in style. The two volumes of which I speak, are the book of Nature and the book of Revelation; and it is impossible for any reflecting, contemplating and observing mind to peruse these two books with attention and care, without being convinced, whatever may be the conclusion on other points to which he arrives, that these are the productions of the same hand. These two books require to be approached, in or-

der to be profitable, in the same spirit; for if that be not a right spirit, both will alike lead to error of precisely the same kind. The Book of Nature, as well as the Book of Revelation, requires to be approached in a spirit of humility: in order to its right interpretation every one must shake off all his preconceived notions, and divest himself of all prejudice; he must give up the spirit of caviling and be prepared to find, both in the one book and the other, most marvellous and inexplicable things. It is precisely because persons have not approached either of these books in a right spirit, that they have fallen into a series of errors—a series of evil consequences—as like each other in the two cases as it is possible for any two sets of moral phenomena to resemble each other. When the Ancients consulted the Book of Nature they read it with arrogant and assuming notions; they read it with an inordinate mental pride, and with the inflated idea that their prodigious intellectual powers enabled them to judge not only what Nature, as revealed in her phenomena was, but what she ought to be. They were accustomed to erect their ideas into a system to which they allowed no exception, and instead of inquiring, as is done in modern physics, with diligence and humility, what are the phenomena exhibited by nature, they laid down certain dogmas or maxims, and endeavored to twist and distort the phenomena of nature, looked at through their false media, to bring them into harmony with their theories—then believed to be infallible, but now known to have been most absurd. In fact it can be proved that a system of the Universe, constructed as one of their sages imagined it must be done, would not endure a single year. The very circumstances which they pronounced impossible are now known to be the causes of the permanency, stability and regularity of the physical world. Now what do we find in all this except the image of what has taken place with respect to the other book? Do we not find that as the Book of Nature, interpreted in an arrogant and haughty spirit, leads to the most absurd and fatal errors, so the Book of Revelation, when approached in the same temper, leads to the same conclusion? But of the evil consequences resulting from a wrong interpretation of the Book of Revelation, it is no part of my province to speak; you have able teachers among you who will give you all needed instruction upon this matter. It is my part to show, that the very evils which these sage instructors endeavor weekly to present, as resulting from the wrong interpretation of the Book of Revelation, flow in the same way from a wrong interpretation of the Book of Nature.

The Ancients, in their inquiries, took up the phenomena of Nature in this way:—They had certain abstract and peculiar notions of physical and geometrical perfection: these they laid down as general axioms, and maintained that whatever was not in conformity with them must not, could not be physically or geometrically perfect. As applied to the motions of the bodies of the universe, one of their axioms was this: that *all perfect motion must be circular and uniform.* They concluded, therefore, that the planets moved in circular orbits and with a uniform motion. Having laid down this as an absolute and universal truth, no one ventured to dispute it, and it was instilled into the minds of the pupils in all the philosophical schools. It continued unquestioned; and it was received as a truth beyond doubt that the principle of circular and uniform motion pervaded the physical world. At this time the book of Nature was not read but guessed at; and from the most hasty and careless observations—if in truth they deserve this name—were drawn inferences of vast generality; and on these unsatisfactory foundations were erected theories of the system of the Universe.

In this state Physical Science was handed down to the dark ages: over these it passed, and at the revival of letters the same maxims were received, and in precisely the same form. But the progress of the Philosophy of the Gospel gave birth to an influence ulterior and exterior to the Gospel itself. Men began to be taught by that system of Moral Philosophy, the true method of considering, arranging and classifying moral truths into a perfect and beautiful system; and they soon learned to apply the same method to physical investigations; and we accordingly find the first development of physical truth appearing just at the time when a right system of moral investigation and inquiry had taken root. First came Copernicus. Up to this time our globe was considered the centre of the physical universe. Copernicus dared to question this; and, by close observation and unfettered inquiry, soon established the theory, which had been formerly hinted at by Pythagorus, that the Sun was the centre of our System. But slight advances were made in these inquiries at the dawn of the revival of letters. Soon after, however, there appeared in the firmament of Science several stars of the first magnitude. Kepler had not as yet completely freed his mind from the thraldom of the ancient system; he would not relinquish the theory that circularity and uniformity were the condition of physical perfection as applied to motion. Still he began to feel that the observations of the Ancients had not been carried to a sufficiently great extent; and he believed it his duty, prescribed alike by reason and philosophy, to open the book of Nature and read it with more careful diligence. He selected as the first subject of his inquiries the orbit of one of the most interesting, because one of the nearest planets to our earth—that of Mars. Kepler carefully observed the ways and goings on of that planet; he watched it from day to day and tracked it in its march through the pathless abyss of space. By a series of observations, easily made, Kepler was able to ascertain from week to week and from month to month, the distance of Mars from the Sun; and he was not a little startled at first finding that this distance was not always the same. As happens in every new discovery, he at first supposed that this was the result of an error in his calculations. This at least is a proof of humility—that the man of science should rather believe his observations erroneous than that he has discovered a new truth. Kepler believed at first that his observations of Mars were wrong, because they showed that this planet was not always at the same distance from the Sun. He soon found, however, that the results of this inquiry had a character which error always wants

—that of uniformity and consistency; that they had, in fact, the character of truth. He soon became conscious of the undeniable and naked fact that Mars was not always at the same distance from the Sun; but clinging, with all the tenacity of prejudice to the old system of circularity, sooner than give up his darling hypothesis, he still assumed that it did move in a circle, but that the Sun was not in its centre. Thus, without sacrificing his theory, he accounted for the fact that Mars was constantly changing his distance from the Sun.

But having once "broken the ice"—having once fallen upon the inestimable practice of careful investigation, instead of blindly following old and rooted theories, KEPLER pursued his observations on the orbit of Mars. He ascertained in each successive position the distance of Mars from the Sun; and was thus enabled, as any one would be, to lay down his orbit upon a chart.—When he had done this, and endeavored to account, according to his observations, for its distance at each successive interval, he found that even a circular orbit would not do it; for the path laid down represented the orbit not as circular, with the Sun either in or out of the centre.—He pursued his inquiries still farther; and after carefully comparing his own observations with all those of his predecessors on which he could lay his hands, he became convinced that the path of Mars, instead of being circular, was oval—was in fact an ellipse. He found, also, that the Sun was by no means in the centre, but aside from this near one end of the oval—that in fact it was at one of the two points in an ellipse called *foci*.

This was a discovery of a capital description. It immediately subverted the received principles of Astronomy. But KEPLER did not stop here. Having once disenchanted himself of his prejudice for circularity, being once convinced that it was an attribute of perfection, he began to consider whether uniformity of motion was a necessary attribute; and from a careful observation he found that the rate of motion which Mars had in his orbit was not uniform. He soon saw that the farther the planet was from the Sun the slower he moved, and the nearer he was the faster he revolved in his orbit. The principle of uniformity was thus at once dashed to pieces as completely as that of circularity had already been. Having satisfied himself that the motion of Mars was not uniform, his next inquiry was after the law, the rule, the physical principles which governed its motion; for that it was subject to some law he had no doubt. He observed the fact that the velocity continually increased as the distance from the Sun diminished, and thence concluded that there must be some close connection between the motion and the distance; and after a patient investigation worthy of modern time enlightened by the inductive philosophy of BACON, KEPLER arrived at another grand law of Nature. If, he found, we imagine a string to be carried from Mars to the centre of the Sun, and as the planet sweeps around this luminary—if we imagine the string to sweep over the space included between the two bodies, that string will move over equal spaces in equal times; not that the *distance* passed over by Mars in his orbit will be equal in equal times, but the *areas*—the number of acres or square miles, if you choose, swept over by the string joining Mars and the Sun will be always equal for an equal time. The number of square miles passed over in one hour, will be the same as the number passed over in the next succeeding hour, and so for all the rest. Thus, instead of the theory of uniform velocity, was substituted that of uniform areas swept over by this connecting line.

Here then was another law of the highest order of generality. But as yet this law had only been established with reference to a single planet. KEPLER had only applied it to Mars. But once engaged in this investigation, his natural keenness of mind led him to obtain the means of extending it to all the planets; and he found that they all move in paths round the Sun, not as his predecessors had always supposed, in circles, but in ellipses with the Sun in one focus, and that they all sweep around the Sun, not with one motion, but with a velocity in virtue of which space swept over, by a line joining them to the Sun, would be equal for equal times. These were two of the first grand results of the inquiries of this great astronomer. Another of scarcely less importance has joined with these to confer immortality upon his name. In comparing the times required by the different planets to complete their revolutions around the Sun, and in reference to the mean or average distance of each, he discovered a still more striking fact: namely, that if you take the number of days required by any two planets to sweep around the Sun, the squares of these will always have the same proportion to each other as the cubes of their mean distances from the Sun. This law was expressed by this familiar formula: that *the squares of the periodic times are in the same proportion as the cubes of the distances.*

These three grand principles have conferred immortality on the name of KEPLER. He was soon succeeded by a mind of vast power, one to which, perhaps, has been ascribed a greater degree of sagacity than it merited, but which was, nevertheless, great indeed. The principle to which I have alluded, as contrasted with a vicious habit of reasoning, namely, an accurate and diligent observation of the course of nature, instead of the vain imaginations of our own minds, commenced by COPERNICUS, was brought into a higher state of activity by KEPLER, and carried to its hight, as I shall hereafter show, by GALILEO.—But a mind soon after arose which, though not the inventor of the inductive principle, still discovered that the careful observation of nature was the only way of arriving at the discovery of philosophical truth. This vast and all pervading principle he saw with a keen and observing glance, and enforced with an eloquence of surpassing power and truth; and thenceforth the inductive philosophy became the basis of all philosophical discoveries. It was seen that careful observation and an accurate judgement were absolutely necessary, and that by such comparison alone, could physical truths be advanced. This mind, as you of course already understand, was BACON's. Those who would disparage the discoveries of modern philosophy declare that the inductive process had long before been practiced. So it

had; but Bacon was the first who embodied it in a system of philosophical method and laid down the general rules by which it may be carried out. He showed the manner of classifying the multifarious phenomena of the outer world—pointed out the method of cross-examining nature herself.—He showed how, when various causes are found to answer for the same effect, infallibly to distinguish the true from the false.

Soon after Bacon had promulgated, in his great and immortal work. the *Novum Organum*, the new method of scientific inquiry, it was applied, with the highest success, by NEWTON. At the early period of his life, he was not a careful and accurate observer; but he was a profound mathematician, and he saw the whole value and force of BACON'S rules, and determined to carry them out in their application to Astronomy; and nobly did he fulfil this purpose. The first fruit of his investigation was this: He took the three great principles of KEPLER—those splendid laws of nature which before were scattered and isolated facts, namely, that the planets revolve in ellipses with the Sun in one of the two foci; that the spaces swept over in equal times, by a line joining the planet to the Sun, are always equal; and finally, that the squares of the times are always in proportion to the cubes of the distances—taking these three principles, NEWLON submitted them to careful inquiry, in accordance with the method of BACON.

The very first conclusion at which he arrived was pregnaut with importance. He showed this: It had been proved that when one body moves round another as a centre, the areas swept over in equal times, by a line joining the two bodies, are equal. This was of consequence only as viewed by this principle, established by the most demonstrative reasoning by NEWTON, that the body round which the other moves is a centre of attraction; and hence springs the conclusion, that the Sun, round which the planets move, must be the centre of attraction for the System. This was the first step in the discovery of the grand principle of gravitation—the central and animating principle of the Solar System.

But NEWTON went farther. He took the other discoveries of KEPLER and said, that, if one body should revolve around another in an ellipse with that other in a focus, whatever be the nature of the force, it must be such that it shall decrease as the distance increases; and this he showed by a most beautiful mathematical demonstration. This was nothing more than an incidental consequence of the elliptical character of the orbit; and as all planets move round the Sun in elliptical paths, with the Sun in one focus, NEWTON proved, by one sweeping proposition, that the action of the Sun upon the planet augmented as the square of the distance diminished. NEWTON thus showed, by KEPLER'S first principle, that the Sun was the centre of attraction, and by the second what is its law, This law of gravitation once established, all the other truths respecting the planetary motions were but corollaries from this. The third proposition of KEPLER was easily disposed of, as it was shown to be only a consequence of the elipticity of the planetary orbits.

Thus sprung out the true method of philosophizing, commenced in the time of GALILEO, possessed and improved by KEPLER, methodized and brought into a regular system of rules by BACON, aud practiced with most illustrious success by NEWTON, that theory of gravitation which is the boast and pride of modern philosophy.

I have mentioned that before the time of BACON the true method of philosophizing had been practised with considerable acumen and success. Among the discoveries thus evolved, one of the most interesting is that of the pressure of our atmosphere. We are all familiar with the fact that air is extremely light—that it is impalpable and quite invisible, and that so attenuated is its nature that it is always taken as a type of every thing spiritual; yet it has been proved that this material, light as it seems to be, has nevertheless, a weight of fifteen pounds for every square inch of surface on which it rests. This fact is of recent discovery, and was unknown to the ancients; and, indeed, from its very paradoxical nature it must have been received slowly and with distrust. The effects of atmospheric pressure were too obvious not to have attracted attention at a very early period. The rush of air into the mouth when the lungs are inflated was obvious, though no satisfactory explanation of the phenomenon was given. The word *suction* was used to express the principle now known to belong to nothing more than the weight of the atmosphere. The first and most familiar effect, from which, indeed, the word is taken, is this: that when the mouth is applied to one end of a tube, the other end resting in any liquid, upon drawing the air from the tube, the liquid rises and fills the mouth. This effect was ascribed to the unknown and imaginary principle called *suction*. The use of the common pump was perfectly known antecedent to the time of GALILEO; it was known that on raising the piston, the water would follow it. The cause which elevated the water was unknown to the ancients, but was explained by suction. This was the vulgar explanation, but philosophers so visionary as the ancients would hardly rest satisfied with the mere word as the explanation for a physical fact. They therefore got up, as was their wont, a high sounding maxim—expressed in poetic phrase, and said that *nature abhors a vacuum*. Thus they personified Nature and invested her with a certain kind of moral feeling—of mingled dread and fear,and this of a space where there is nothing. This, like the circular hypothesis, was laid down as an incontrovertible maxim, and no one dared to dispute the assertion that Nature abhors a vacuum. This accounted, and to them satisfactorily, for the elevation of the water in the pump. The pistons when raised, would cause a vacuum in the pump if the water did not follow it; but Nature abhors a vacuum—therefore the water follows it. This was their demonrtration, and it was received and taught by sages quite as wise as the philosophers of the present day. It happened during the life-time of GALILEO, and while he yet had a name and considerable authority in Italy, that a pump-maker near Florence had occasion to sink a cylinder to an unusual depth. Having sunk the pump, he attempted to draw up the water to the height of about forty feet; but he found that it would not follow the piston farther than about thirty-two feet. At this he was tremendously puzzled, and had recourse to the greatest philosopher of the day, GALILEO, for an explana tion. Said he, 'What is this?—we have here

most extraordinary circumstance, to be sure; the water will rise to the height of about thirty-two feet very readily—but beyond that it will not go at all.' The tradition is that GALILEO made answer that 'the abhorrence of nature to a vacuum is limited; that she abhors c vacuum up to thirty-two feet, but that beyond that she has no objection to it.' This answer of GALILEO is believed by many to have been ironical, and that the philosopher really had some inkling of the truth, but that not having quite satisfied himself of it he got rid of the *bore*—of the pump-maker and not of the pump—by this evasive answer. Others declare that he meant what he said, and that he really believed that Nature's abhorrence to a vacuum was limited to a hight of thirty-two feet.

Be that as it may, it is certain that GALILEO never gave the true explanation. He left the fact where it was. He is said however, to have given a reason for it like this: That the base of the piston had a specific attraction for water; but that this attraction had not sufficient energy to raise the fluid higher than thirty two feet. I do not know whether GALILEO ever held this opinion, poor as it is or not; I have never seen it in his works. In this state the matter remained till after the death of GALILEO. He had a distinguished pupil named TORICELLI, and to him he bequeathed the honor of the solution to this question. After his death, TORICELLI applied his mind vigorously to it and produced one of the most beautiful solutions ever given of any physical problem. He at first suspected that the pressure of the atmosphere upon the surface of the water outside of the cylinder, was the agent which forced the water within to follow the piston; he saw that if this was so, this pressure must support a column thirty-two feet in hight, and he proposed to submit this to accurate experiment. He said: Now if it be the pressure of the air which sustains this column, it sustains it merely as it would a given weight. Now if I can manage to get another fluid, as oil, which has a different weight from water, then, if my theory be correct, the pressure of the atmosphere must sustain a column of this fluid differing in hight from that of water; if the fluid be lighter the column will be higher; if heavier the column must be shorter. For convenience sake he used quicksilver in his experiment, which is thirteen or fourteen times as heavy as water, and therefore ought to give a column of only about thirty inches. To submit this to the test, he took a glass tube open at one end and closed at the other; this he filled with quicksilver, and placed the open end in a vessel of the same liquid: Now, said he if the pressure of the air support a column of water it ought to support a column of quicksilver of the same weight, and this he found to be precisely the case.

Now you would suppose that such a demonstration as this of a fact so important and interesting would satisfy every one. But you must remember the trammels of prejudice—the desperate tenacity with which men cling to long received opinions. This demonstration did not convince every one; on the other hand it convinced very few. Some few assented to it, while the majority thought that it had not been sufficiently proved. Some one was needed to apply to the solution the final test—to apply what BACON calls the *experimentum crucis*. This one was soon found in PASCHAL, with whose name and works you are all doubtless acquainted. Although he firmly believed that the explanation of TORICELLI was the right one, he saw that an experiment was still wanting which should put it past all doubt, and this *experimentum crucis*, or experiment which, when there were two or more methods of explication apparently of equal validity, should point out decisively the true and the false, PASCHAL prepared to supply. He reasoned thus: If the weight of the air be the principle which sustains the mercury in the tube, if I carry the tube up in the atmosphere to a great hight, as a portion of the sustaining air is thus left behind, the mercury ought to fall; but if some other cause be the true one, it will remain at the same hight. PASCHAL accordingly wrote to a friend living near a mountain in Auvergne, in France describing the experiment he wished to make; 'and now,' said he, 'as I have no convenient means for accomplishing this, pray fill a glass tube with mercury in the manner described, take it up to the top of your neighboring mountain, and see if it do not fall several inches.' His friend obeyed his wishes, and found the result just what had been anticipated. As he carried it toward the top the mercury continued to fall. PASCHAL soon after repeated the experiment on one of the high towers of Paris—that of Notre Dame, I believe—and to his complete satisfaction. A happier or more beautiful illustration of what BACON meant by the *experimentum crucis* can scarcely be found in the whole range of Physics.

The barometer, the principle of which I have thus been describing, is, as you are all aware, often called a weather glass. It is not my purpose at present to go into a description of the barometer; but I may speak briefly of what the barometer does really indicate, and what it does not. And in one word I may say, that it indicates nothing else than the weight of the atmosphere. If taken to another part of the same building—if it be taken to the top of a church tower, the mercury in it will fall; for the weight of the air it measures is now less than before. If it be kept in a given place, the mercury will stand at different hights at different times from the operation of various causes. Upon the barometers usually constructed, are engraved, at different hights, the different terms, "fair," "foul," "rainy," &c, as if the condition of the atmosphere was indicated or predicted by the hight of the mercury. That this is not the case any one will be convinced by considering that a barometer may indicate fair weather at the bottom, and foul weather at the top of a church steeple, since the hight of the barometric column will be different for the two positions.

What, then, are the indications of the barometer to be relied on? This is not readily answered, as none of its indications are invariably true. All that can be said of its promptings is, that certain effects will probably follow certain changes. As a general rule, if the barometer suddenly falls, foul weather may be soon expected; and if it suddenly rises, fair weather will probably follow. These rules, however, are greatly modified by various circumstances. In summer, if it suddenly falls, thunder may be expected; and if in winter, a frost. These can only be relied on as probable

indication; as to the terms upon the barometer, they are perfectly futile, and should not be there.

DEW.

A second and striking natural phenomena selected for analysis this evening is that of dew. This is rendered celebrated for the extremely beautiful investigation out of which the discovery of its theory arose. We are indebted to an eminent natural philosopher, Dr. WELLS, for this theory, and he developed it complete in all its parts; and I am sure that you will consider it most beautiful and satisfactory if I can succeed in making it as clear as I ought to do. When any natural phenomena is to be examined, and a clear cause assigned to it, the first duty of the inquirer—though unfortunately it is one not always observed, is to obtain a clear conception of the thing or the effect itself, which we propose to examine. In an inquiry into the nature and origin of dew, it is necessary first to distinguish it from other things which may be supposed to resemble it more or less. For instance, it is not rain: rain falls in drops, regularly formed in the air before they are deposited in the earth's surface. Dew does not: nor is it ever known to exist in a liquid state until it touches the body on which it is deposited. Rain falls: dew collects. Rain is never driven upon the under side of a leaf: dew is as likely to be found there as elsewhere. Nor is dew the same as fog; for fog never exists unless there be dampness in the atmosphere, while dew is never found unless the sky be clear and the air dry. You see, then, what dew is as distinguished from other objects. We find it existing in objects upon the surface of the earth, a clear, pellucid drop; but in order clearly to investigate its character, one must open the Book of Nature and diligently read its pages. The ancients set up lofty theories, which they only thought of supporting: the modern philosophy submits Nature to the severest examination, and extorts from her the truth. When any question is proposed for investigation, the first and most natural inquiry is, whether there be any other similar effects; for if there are, they are probably owing to the same cause; and if this be known in the latter case, of course it is in the former also.

Every one must have observed that if a glass decanter, filled with cold water in a warm day, be set upon a table its surface will soon be covered with a cloudy film, which soon forms itself into large pellucid drops, which have all the characteristics of dew; now of this appearance upon the decanter we perfectly know the cause; the watery particles which always in warm weather are suspended in the air, are condensed by the coldness of the water and deposited upon the surface of the decanter where they come in contact. Now, said Dr. WELLS, if the phenomena of dew so closely resembles this which I witness upon the decanter, why may it not have the same cause? He, thereupon, endeavored to trace closely the analogy between the two. He knew that moisture was deposited upon the decanter because the decanter was colder than the air around it. To bring the bedewed object into the same predicament it is incumbent to show that this object is colder than the surrounding air. If this be proved, one real step will have been made in the investigation. This Dr. WELLS clearly saw; and he likewise saw the necessity of applying to nature for this proof. He took two thermometers and plunged one among the leaves of plants in the night time when the dew falls; the other he suspended in the atmosphere some two or three feet above the leaves. He watched these two thermometers during the night, and found that the one among the leaves was 20° lower than the other. Here he had made one real step in the investigation; the analogy of dew and the drops on the decanter thus far was complete.

Now, said Wells, we have got this important point; the dew is deposited upon the plants because they are colder than the surrounding air. The air always sustains a quantity of vapor; and the plants, having lost their temperature, operate just as did the decanter of iced water, and a deposition of dew takes place. But another important inquiry remained behind. What was it that cooled the leaves? Surrounded by warm air, why should they be cold? Mark the beautiful conclusions to which this inquiry gave rise. To answer the question the first thing was to ascertain the physical circumstances under which they did not become cold; it is not on every summer night that dew is deposited; and, said he, if I find one of them, I will see whether the plants are then cold. He made the trial, and found that the plants and air were then of the same temperature.

Another striking difference between dew and rain or fog is this: that it selects the objects on which it is deposited. It will not fall on all alike. Now Dr. Wells said that in order to trace the phenomenon to its original cause, it is necessary to see what is the physical character of the bodies on which it is deposited, to distinguish them from those on which it is not. It falls by preference on the flowers, blossoms and leaves of plants.—Look at the bell of a summer flower, and you find hanging dew-drops like pearls upon it. Look upon the earth, the stones and flags, and you find there is none deposited. What is the physical characteristic which distinguishes these plants from other objects? Dr. Wells decided on inquiry that dew fell most copiously upon those objects which were the best radiators of heat; while upon those which scarcely parted with their heat at all, there was none to be found. Here then was another important step, and here, said Wells, I have the key by which I shall open this secret and find out what is the cause of dew.

There was another inquiry: Dew not only elects certain objects on which to fall, but certain nights during which it is deposited. The same body which is bedewed this morning, to-morrow morning shall be perfectly dry. It is found that dew never falls on a cloudy night; it is absolutely essential that the night be clear. Thus there must be something in the clouds which is connected with the deposition of dew which the clear sky has not; and it must have some reference to heat, for this is the agent in operation. Again the principle of radiation suggested itself. The clouds are good radiators, while the sky radiates no heat at all. We are then brought to this; the deposition of dew requires that the objects on which it falls be good radiators, and that the heavens

above radiate not at all. It is clear, then, what makes the plants twenty degrees colder than the air; they themselves are continually throwing off heat, while from the heavens they receive none in return. Of course they must fall in temperature; then the vapor in the air is condensed upon them just as it was upon the decanter of iced water. This was the explanation of Dr. WELLS of the theory of dew, and one more beautiful is scarcely to be found in the whole history of modern discovery.

It was necessary, however, that some way be devised for the trial of the *experimentum crucis* with reference to this theory. For this purpose, Dr. W. selected a silver cup, which is a bad radiator, and placed it in a glass basin, which is a good one. He exposed them to a clear sky on a summer night. In the morning the glass basin was covered with dew, while the silver cup was perfectly dry. He next placed a glass cup in a silver basin; in the morning the cup was bedewed, while the silver basin was dry; upon a cloudy night no dew was deposited upon either. This left no possible room for doubt that he had discovered the true theory of dew.

In bringing before a public, assembled not in colleges or universities, but in this way coming directly from the ordinary pursuits of business, examples of the extreme rigor of modern scientific investigations, I trust my lectures will be attended with one good effect, in addition to the mental discipline every one must receive from listening to such reasoning; I trust I will give you a stronger faith in the wonders every day discovered in science. You hear detailed the main facts, the results of scientific discovery; and you are apt, I fear, to think that such theories must rest on but slender evidence; and that philosophers are induced by their ardor and enthusiasm to give to the results of their investigations greater faith than they are entitled to. But I trust that in giving to you examples of the rigorous methods adopted to arrive at scientific truth, which are applied to all discoveries, you will see that philosophers do not credit a thing merely because it is marvellous; but that in proportion as its wonderful nature is increased so much the more rigorous is the demonstration required.

SCIENCE AIDED BY ART.

On a former evening I had occasion to bring before you some of the wonderful discoveries of modern Astronomy, and I gave a general outline of the kind of reasoning and observation on which these discoveries are based. But I found it necessary to draw upon your faith for the extreme accuracy of which certain observations are susceptible. I was obliged to assume that astronomical instruments could be constructed capable of making observations within certain limits; and as I then stated, instruments are capable of measuring a variation of a second.

I do not propose now to go into a detailed description of these instruments, but only to give one or two examples of the manner in which art assists science in conferring this extreme accuracy on philosophical investigations. The instrument with which nearly all accurate measurements are made, is called the *mural circle*. In the great observatories at Greenwich and Paris, the ground is first excavated until a solid rock is found. Upon this is built a tower of solid masonry, so that it may be affected by no jarring motion of the earth. Around this tower is the building of the observatory. All communication with the tower or floors, is cut off, so that it shall be affected by no motion but that of which the whole earth partakes. Against the face of this wall the *mural circle* is placed; those at Greenwich, and, I believe, at Paris, are brass circles, some six feet in diameter, accurately suspended on an axis, fixed in the wall, about which they turn. To the circle—the line of its direction passing through the centre—is fixed a large telescope. When this is brought to bear upon a star, across the field of views is extended a system of fine lines—so fine, even, that when magnified by the power of the telescope, they appear like the finest hairs. It was long a *desideratum* to obtain some durable substance which should answer this purpose. The first artist to whom application was made for these threads was one taught, not by man, but by a higher power: the spider's web was for a long while used for this purpose. But it was evident that it was perishable, easily broken, and liable to many other objections. A more permanent, and indeed, finer wire was desired. But of these threads I will soon speak more at length. For the present we will return to the mural circle.

When the telescope had been brought to bear upon the object as accurately as desired, it is fastened in that place firmly by a 'clamping screw.' Then to make the observation, this circle, six feet in diameter and about nineteen in circumference, is divided into parts so extremely minute that they cannot be read with the naked eye. In order to be visible they must be subjected to a powerful miscroscope. But the angle almost always lies between two or three divisions, minute as they are: it is a mere casuality if it chances to fall exactly upon them. It is incumbent, then, to know, not only the size of these invisible divisions, but even fractions of them. This is accomplished by a most beautiful contrivance. A fine wire is carried parallel to itself by a screw so fine that to move the wire over one of these invisible divisions sixty revolutions of the screw are required; so that one revolution will move the wire through one sixtieth part of one of these invisible divisions. Now see the accuracy to which we have arrived. The limb in the first place is so fine that it is only visible through a microscope; this is subdivided by means of a screw, sixty turns of which move the wire over one of the divisions. This will be sufficient to show you that there are in the hand of the astronomer means of accurate measurement not possessed by any other department of Science.

I will now return to the wires extended across the field of view in a telescope. The discovery of those now in use is one of the most beautiful of modern theories; and for it we are indebted to WOLLASTON, whom I have often mentioned, and whose acquaintance and friendship I am proud to have enjoyed while he was living. He saw the imperfections of the spider's web, and he felt that art ought to furnish something finer, far more durable and in every way to be preferred; he resolved to contrive a method of doing this, and contrive it he did, in the mode I will now explain.

Wollaston took a cylndrical mould of very small diameter. Through the centre he extended a fine wire of platinum. He then filled the mould with melted silver; after allowing it to cool, he withdrew it, and thus had a rod of silver with a rod of platinum in the centre. The silver was of ten times the diameter of the platinum wire.

Having this, he submitted the compound wire to the process of wire-drawing, with which I presume you are all familiar. It consists of drawing the wire successively through holes of constantly decreasing size. In this process Wollaston knew that the mutual adhesion of the two was so great that the silver could not be wire-drawn without wire-drawing the platinum also; and that the proportion in which they would be drawn out would remain the same. He accordingly went on until he had reduced the silver wire to as great tenuity as it would bear—until it was .03 of an inch in diameter. Now, said Wollaston, I have got a thread of platinum within this silver wire which is only one-tenth of its thickness; it is, then, only .003 of an inch in thickness, for the same proportion was preserved through the wire-drawing process.

But how to get it out? He bent the wire into the form of a U, with a hook at each end; he then suspended it in a vessel of acid which was capable of acting on silver, but not on platinum. The result was, that the silver was soon dissolved, while the platinum wire remained. He had then a wire of platinum so fine that the naked eye could not see it.

This seemed far preferable to the spider's web for purposes of telescopic observation. I said that its thickness was only the .003 of an inch. Wollaston himself states that he obtained a wire only one eighteen-thousandth part of an inch in diameter. You may judge, then, of the extreme degree of tenuity given to this metallic wire by considering that of a piece of platinum as large as a die, used in backgammon, a wire which would reach from here to Charleston might be made.

LECTURE VI.

SCIENTIFIC DISCOVERIES.

Ladies and Gentlemen: In the programme of a former lecture—the one in which the subject was the modes in which Art imitate Nature—allusion was made to the forms affected by the feathers of birds, the tails of fishes, and the form of fishes themselves. Although it is a subject perhaps somewhat trite, it still never fails to excite a certain degree of curiosity to observe the extremely beautiful manner in which the mechanism of these organized bodies is found. It is recorded of a very eminent preacher that on one occasion he commenced his sermon by this exclamation: "O God! what a Geometrician art thou!" which was followed by a most beautiful and eloquent essay upon the peculiar mathematical principles developed in the structure of organized bodies, and in many other natural objects. At one time during the last century it became a problem of no little difficulty, and of considerable mathematical interest, to determine the best form that can be given to the sails of a wind-mill, according to their varying distances from the revolving vanes, and likewise from the centres of revolution. It will be recollected that the arms revolve in a plane at right angles to the direction of the wind. It is obvious that if the sails were in a position exactly perpendicular to the arms, the wind would not strike them to any advantage, and there would be no motion. By placing them in a position partly perpendicular, and partly oblique, a revolution would follow more or less rapid according to the degree of obliquity, some angles giving more velocity than others. The velocity will also evidently vary according to the distance of the sail from the revolving arm and from the centre of revolution. The question thus became a very complicated geometrical problem, as it proposed to determine the best possible position at every variation of distance and at every part of the arm. After all their labored solutions, and when the best possible form had been attained, it became evident that the result was a glaring copy of the wings of a bird, and that the problem had long before been solved by the 'great Geometrician' alluded to by the preacher I have mentioned.

Another mathematical problem of considerable difficulty for some time was to determine the *solid of least resistance*, as it was called: that is, to determine the form which a solid body must have to move through a fluid with the least possible resistance. It is evident that if a body, with its flat surface to the fluid, be pressed through water, it will encounter a very considerable resistance, and the problem engaged the attention of Newton, and the other celebrated mathematicians of the day, to determine the form of a solid which should encounter the least. After it had been solved, the result showed that the form given was that of the fish—so that the philosophers in solving it had only followed in the steps of Him who created the fish. In the tail of the fish is also found an example of art copying nature; as it was found that a subaqueous oar fastened to the stern of a vessel and playing in the manner of a fish's tail, was a very efficacious propeller of a ship. The particular motion required was communicated by a steam engine. You may recollect also the resemblance between the leaves of Ericson's propeller, and the tails of birds.

Another subject which I should be glad to discuss is that called *Electrotype*. It was found by Sir Humphrey Davy (and most fruitful did the discovery prove in his hands,) that certain bodies have a specific attraction for the opposite poles of a galvanic battery; and from this resulted the process called 'Electrotype,' in which, by collecting the metallic element at these two poles, a form will be obtained like a casting of the particular object. I have been shown a very curious model of this kind by Mr. Frost, of Brooklyn, which I hope to be able hereafter to exhibit to you.

Another topic, announced in the programmes but hitherto untouched, illustrates in a remarkably beautiful manner the way in which the various branches of science and arts minister to each other—each receiving from the other some unexpected benefit. The history of the discovery of *Iodine* is extremely interesting. It was about the year 1812 that Courtois, a French manufacturer of salt-petre, I think, found that the liquid residuum of the process of his manufacture produced a corrosion of the metal vessel—which I

think was copper. That this proceeded from the action of a chemical agent he had little doubt; and he accordingly took it to a celebrated chemist, M. CLEMENT, who, on examining it, found that it was a principle not before known in Chemistry, —that it was a substance new to him. It proved to have certain properties bearing a close resemblance to the Chlorines, and in some respects to oxygen. At first, CLEMENT did not succeed in fully investigating the problem or in settling its nature; and it fell into the hands of GAY LUSSAC and Sir HUMPHREY DAVY. The result was the discovery of a chemical substance not only new but simple—that is, of a substance not *at that time* known to be decomposable into constituent parts. By no known tests could it be resolved into two or more substances; this, in the strict nomenclature of Chemistry, is the meaning of the term *simple*:—that by no hitherto discovered test can it be decomposed, although future discoveries may prove it a compound. When DAVY and GAY LUSSAC then discovered that Iodine was a simple substance, all they did was to discover that it could not be resolved by any then known tests. It was taken, then, as one of the simple substances, and showed a striking analogy to others, such as Chromine, Bromine and Oxygen.

This discovery remained for some time on the surface of Chemistry—barren as to any useful purpose, though Iodine was found to have a strong affinity for the metals, and to combine readily with various simple substances. The discovery of the principle, as I have said, originated in the process of manufacture of sea-weed, which was used by COURTOIS. He soon traced it to other marine productions, to sea-water, salt mines, &c. It was soon after discovered by an eminent philosopher, still living at Geneva, Dr. COINDET, who had given considerable attention to Iodine, by an instinct characteristic of great minds, that it might be an active agent in certain remedial processes. It was known that in the Alpine dictricts a disease called the *goitre*, manifested itself by frightful swellings on the glands of the face and neck, had been cured, not by the regular practitioners, but by the village empirics, by the use of burned sponge. Dr. Coindet, knowing this fact, suspected that the virtue of the burned sponge might be owing to the Iodine contained in it. He tested it by experiments in his practice, and since that time Iodine has figured prominently in the practice of medicine.

SOUND.

Another subject to which I will ask your attention is that of *Sound*, which presents many interesting circumstances, and is capable of easy and intelligible explanation. Sound is produced by certain pulsations, or undulations, imparted to the atmosphere by the sonorous body, and conveyed to the drum of the ear, which is constructed with a view to a susceptibility of their vibrations; thence the impression is conveyed to the sensorium, and the sensation of sound is obtained. The fact that the atmosphere is really the medium through which Sound is conveyed is easily proved. If a bell be suspended in the receiver of an air-pump, while the air continues in the receiver, the sound of the bell, if struck, will be audible; but if the air be exhausted, it will diminish in intensity until it finally becomes quite lost. To make this experiment successfully, the bell must be suspended by some inelastic substance which will not convey Sound. That Sound depends on the density of the atmosphere for its loudness, is shown by the fact that when SAUSSURE ascended Mount Blanc, he found it extremely difficult to make himself heard by his companions if they were at any little distance from each other: and the same thing has been proved in the case of the Himmelaya Mountains.

But although air is a medium for the transmission of sound, still it may be conveyed in other ways; and a beautiful experiment tried at Paris shows that metal is a better conductor of Sound than the air. A very long metallic tube was provided, and one end of it was struck, while an ear was placed at the other end to hear the sound.—Two distinct sounds were heard—one conveyed by the metal, and the other by the air—and the first was heard the soonest by a certain short interval. All substances which will convey Sound must be more or less elastic; and in proportion as the elasticity produces regularity of vibrations, it is found that it acquires the character of a musical sound.

In the examination of Sounds there are three distinctions made, which are known by these names, (although we have no very good English terms for)—the *pitch*, the *intensity*, and the *quality*. The *pitch* of a Sound is what is called the *note* in music; for instance, the note produced by striking a particular key of a piano-forte is of a certain pitch—either higher or lower. The *intensity* of a Sound is a name merely expressing the degree of loudness; it is evident that a sound may have the same pitch as another, and still be of a very different degree of intensity; thus, the same note of a piano-forte if struck first with the soft pedal up and then down will be of the same pitch but of different intensity. The third character of Sound is its *quality*, which is hard to describe, as we have no good terms to express what it means. I can only explain it by an example. If the string of a guitar be struck, it will produce a certain note in the musical scale; the string of a piano-forte, if struck, may produce a note of the same pitch and the same intensity; but the two will still be of very different qualities. The same note on the flute, or a clarionet and on a hautboy, though it may be of the same pitch and intensity in all the cases, will be of a very different quality. If the science of acoustics were sufficiently advanced, we ought to be able to define precisely the mechanical circumstances by which all these are distinguished from one another—to state the number of vibrations in the air requisite for each. If a hammer be struck upon an anvil, upon a stone, and upon a piece of wood, the sound in the three cases will be of different qualities; but if you ask the scientific or mechanical distinction, we are unable to answer; though, if the science were sufficiently advanced, we ought to be able to tell the number of vibrations in each case; but at present this cannot be done.

Sounds, in order to be musical, must be produced by vibrations which must be continuous and regular; they must be caused by a continued succession of equal and regular vibrations of the air caused by the sounding body. Unless this be so, the sound will not be musical. The object in the

construction of musical instruments, then, must be to procure the bodies which will cause, under all vibrations, an equal number of vibrations in an equal time. The first and most simple of these bodies and the earliest employed were musical strings, and as these appear the easiest way of explaining to a popular audience the theory of musical Sounds, I shall continually refer to them. If the string of a violin or guitar be drawn from its position of rest and allowed to return, it will not stop at its position of rest, but will pass beyond it, then its elasticity will cause it to pass to the opposite side, and thus it will continue to vibrate from one side to the other; and every vibration of the string imparts to the air a corresponding pulsation. The string at first will pass to a greater distance from the position of rest than afterwards; but the time required for each vibration will be equal, whatever be the length of the vibration. So long as the string goes on in this vibration, pulsations are communicated to the air and thus conveyed to the ear: and the number of vibrations for each second will always be the same for the same string. That each vibration, whether longer or shorter, will require the same time, will be evident from this consideration: that when we draw the string from its position of rest, in the first place, the force with which it tends to return is much greater than when, by the resistance of the air, the vibration of the string is diminished: it will consequently return with greater rapidity in the former case than in the latter. It is found that this admits of strictly mathematical demonstration; but this is not of a character to be exhibited to a popular audience: it is sufficient to state that it is capable of geometrical demonstration—that the time taken in the one is precisely equal to that required in the other, because the greater the space to be passed over, the more energetic the tendency to return, and consequently the velocity is greater in the same proportion.—Thus when we pass the bow of a violin over one of its strings, whether with greater or less force, whether the vibrations produce a loud, or light and whispering sound, still the number of vibrations for each second is in both cases the same. The sound will not differ in pitch but in intensity. In the same way, if the string of a piano-forte be struck with greater or less force, the divergence in the former case is greater than in the other, and the notes therefore differ in loudness. The same principle applies to all the modifications of musical instruments, if they be played as they ought to be, which, to say the truth, is not always the case—especially with the flute. A judge of musical instruments has said that it is physically impossible to play the flute in tune; that if one note be correct another will not; and this is, to some extent, at least, perfectly true; for the volume of air put into vibrating motion, depends to no inconsiderable extent, upon the *embouchure* or mouth, which is constantly varying: so that the same fingering may actually produce different notes in different cases. This is an incurable defect which attaches to the flute—that the vibrations produced are uncertain. This is not the case with stringed instruments.

Having thus explained in a general way the principle on which the musical character of Sound depends, I will now endeavor to explain the circumstances on which the effects of combined Sounds depends; this is simple and admits of strictly scientific investigation. If a musical string be divided in the centre—for instance if the string of a violin, between the bridge and the nut, be bisected and stopped in the middle—and if the one half be struck, the note produced will be an octave to that of the whole open string. This is a natural fact to be decided by the ear. It admits of mathematical demonstration that the number of vibrations produced in each part of the string, will be double that of the whole string; or in general terms, that so long as the tension and material of the string are the same, every change in length produces a corresponding change in the number of vibrations: as the length is diminished the number of vibrations will be increased; if one half the string be struck the vibrations will be twice as many for the same time, as in the whole string; if one-third be struck the vibrations will be three times as rapid, &c. The division may be carried on to any extent and the number of vibrations in a given time will vary in inverse proportion to the length of the strings.

This leads to a striking and interesting conclusion respecting the principles of harmony. If you sound a note with its octave the ear readily distinguishes them, so complete is the harmony. But it may be asked, why should the simultaneous production of these two sounds produce an agreeable sensation, as it is known to do? This is easily explained. You will all admit that one half of the string if struck will vibrate twice as fast as the whole; now if both be struck at the same time the ear listens to both together and its mechanism is so constructed that it is capable of receiving both effects without interruption. But how is this possible—that the ear drum should receive two systems of pulsations, the one being twice as fast as the other? This question belongs to a large class of phenomena, and admits of illustration by the principle of waves. If a pebble be dropped into a smooth pond of water, it will cause a circle of waves to spread from the centre at certain intervals from each other. Now if, while this one system of waves is propagated, another pebble be let fall in the same place, that will produce a different system of wavelets—either greater or less; yet the two will go on to be spread over the surface of the water, nor will they interfere with each other—the one passing over the other. Yet the eye perceives both distinctly. This is the case with the pulsations in the air, where there are two different Sounds. The number of pulsations for each minute, received by the ear from the long string is only one half of the number received from the short one; and the intermediate pulsations are broken. The consonance of the two Sounds depends on the coincidence of every other vibration in the short string with every vibration in the long one.

These observations lead to an inquiry as to the power we possess of counting the number of these vibrations. I have told you that this admits of strict mathematical computation; but as this cannot be exhibited profitably to a miscellaneous audience, we must have recourse to another means of determining it, which does admit of popular explanation. I do this the more willingly as it will bring to your notice a most beautiful little

philosophical instrument, called the *Sirene*, by which the vibrations may be counted and registered, invented by a French noble, the Baron CAGNOND DE LA TOUR. It may be described in this way. Suppose a metallic tube, open at the end like an organpipe, to ascend to an orifice through which the air in it may make its escape. Two circular plates, one directly behind the other, are so arranged that the air from the tube can escape only by passing through holes placed around the outside of the circles at short intervals from each other. One of these plates is fixed in its position; the other, by revolving, causes the holes to pass—first, directly before the holes in the other, (in which position the air may escape,) and then before the spaces between the holes in the hinder circle, (when the air is interrupted and cannot escape.) Imagine the circle then to be turned rapidly; the air will escape when the holes in the two are directly opposite each other, and be interrupted while passing from one to the other; so that it will be discharged in a series of puffs; and if the motion of the revolving plate be regular, the pulsations of the air will be regular; and if the motion be sufficiently rapid, a musical sound will be produced. Now, if by any means, the motion of the plate can be regulated, we can make the pulsations succeed each other at what rate we please; and thus, if it be rapid, the note produced will be high; if slow, it will be low. But something still remains: we must have the means of counting the number of pulsations in a minute; and nothing can be more simple or beautiful than the arrangement for this purpose. On the axis of the circle is a toothed wheel, which imparts motion to hands which play upon dials like those of a clock. For every revolution of the circle the hand passes over one of the ten divisions on the dial; so that in ten revolutions of the circle, the hand will have passed completely round the dial. Now another dial is so connected with the first, that one complete revolution of the hand of the first shall cause the hand of the second to pass over one division, and ten revolutions of the first will cause one of the second. There is still a third dial, one revolution of the hand of which corresponds with ten of the second, of course with one hundred of the first. A fourth is also provided, one revolution of the hand of which corresponds with ten of the third, one hundred of the second, and one thousand of the first. It will be seen that the system of decimal notation is here completely preserved. Now in order to count the number of revolutions of the circle, and, consequently, the number of vibrations of the air, all we have to do is to watch the hand on the fourth dial plate. Let us see how this is to be applied. If the tube be connected in any way with the bellows of an organ, so that air may be driven through it, it will cause the circle to revolve with greater or less rapidity, and a musical sound will be produced. The question is to know how many vibrations are necessary to produce, for instance, the note A, or any other note of the musical scale. We have nothing to do but to go on giving greater and greater velocity to the circle, until the musical sound produced is found by the ear to be the note required; then observe the number of revolutions of the dial hand for any period of time—as for a minute—and you have the number of vibrations necessary for any note in the musical scale. If you wish to test the principle of the octave with this instrument, in the first place give a motion to the circle sufficient to sound the note, say D, and note the number of vibrations; then sound the octave of the note, and you will find the number of vibrations to be just double that in the former case. Hence then it is proved by experiment that the octave vibrates twice as fast as the elementary note.

Having thus endeavored briefly to explain the principles on which depends the agreeable sensations produced by the sounding of a note with its octave, I will now present one or two other combinations; and I hope there are a sufficient number of persons who know enough of music to follow me upon the staff, to render the explanation, which must be somewhat dry, not altogether uninteresting. I explained that every other vibration in the octave corresponds with every vibration in the elementary note. If, now, you take the two octaves above the first you have four notes rising one above the other; the second vibration in the second note, the fourth vibration in the third note, and the eighth vibration in the fourth note, will coincide with each vibration of the first or elementary note, and so on. The series of octaves produces its agreeable effects then by the concordance of the first, second, fourth, eighth, sixteenth, &c., vibrations with each vibration of the elementary note.

When the string is divided into thirds, and two of them are sounded, we get what is called a *fifth;* and the agreeable sensation in this case arises from two vibrations of the fundamental note corresponding with three of the other. Consequently every second vibration of the one occurs at the same moment as the third vibration of the other, and the concordant vibrations are the second and third on the two strings. The two adjacent strings of a violin sounded together give a *fifth*, which is a peculiarly rich and harmonious chord.

Next to the fifth is the *fourth*, in which there is a different concordance of vibrations. The concording vibrations here are the third and fourth, three of the elementary notes being simultaneous with four of the other. That is what is called a fourth. Other combinations succeed those which might be analyzed in the same way; but it may be sufficient to state the general principle, which is this: the chords are rendered harmonious by the frequency of the concordant vibrations; the concordance of the second and third being more frequent than that of the third and fourth, the former chord, which is called a *fifth*, is more harmonious than the latter, which is called a *fourth*.

I may mention here something of the effect of striking a bell which gives rise to another instance of musical sound. Suppose the bell be represented by a circle; the tongue striking on one side of it throws the side from the centre and converts the circle into an oval; the returning motion causes the bell to receive the form of an oval in a direction at right angles to the first, and thus we have a succession of ovals formed by the vibrations of the bell—at right angles to each other.—This depends on the same principle that has been already explained.

The instrument, invented by the Baron CAGNOND DE LA TOUR, has been applied, not only to the science of music, but also to Natural History: you all have noticed the humming noise produced by insects, as mosquitoes or gnats; this is caused not by the organ of the voice, but by the flapping of the wings, and all that is necessary in order to count by the *Sirene* the number of vibrations for each second of the wings of an insect, is to let in the wind into the tube until the musical sound is in unison with that of the mosquito or gnat; then note the revolutions on the dial and you have at once the number of the flappings of the insect's wings. This is an extreme and striking example of which one branch of science often ministers to another. There are many cases of this, but none more striking than the one just noticed.

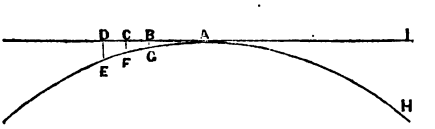
VIBRATIONS OF THE RETINA.

A wish has been expressed that I should explain more fully a circumstance to which I alluded briefly in a former lecture; namely, the effect produced on the retina of the eye by light. I explained the principle of light as established by modern physics, and the impression of the different colors upon the retina. The physical principle on which light depends was for a long while, and is still in dispute among philosophers. One sect maintains that light is a physical emanation from a luminous body, which passes through space at the rate of 200,000 miles in a second, reaches the eye, affects the retina, and produces an impression in the mind. They hold that the white light of the sun is composed of particles of different kinds, each producing the notion of a different color. This theory has been in dispute, and is found to be insufficient for the explanation of certain phenomena discovered by modern science. This theory, however, was maintained by NEWTON.

Another theory, which is now generally received, is this: It supposes that the whole universe is filled with a fluid called *æther*, extremely subtle and elastic; and that the luminous body produces the effect by imparting to this æther a certain pulsation, precisely similar to that of sound, to which I have already referred. These vibrations are supposed to be transmitted to the eye with the velocity already mentioned. Having entered the eye, it causes the retina to vibrate just as does the ear-drum in hearing, only these pulsations are infinitely more rapid and delicate than those of sound. This theory maintains that the colors of the spectrum, as red, orange, blue, &c, are nothing more than the effects of greater or less rapidity of vibration. If the retina pulsates at one rate, red will be seen; if at another, blue, &c.—This is the general outline of the theory now generally received in reference to the *Corpuscular* theory, which was held by NEWTON, but which is insufficient to explain many of the phenomena observed, which the undulating theory perfectly accounts for. What all these phenomena are, I cannot explain in full; I will, however, mention one of them, which is among the most remarkable.

If two beams of light be admitted through small apertures in a screen, and be made to cross each other under certain circumstances, so that they fall upon the same point, you would naturally expect that that point would be twice as light as if but one beam fell upon it. According to the Corpuscular theory, which holds light to be a material substance, the more of it there was accumulated upon any point, the greater would be its illumination. But it has been shown by modern science that, instead of this result, the two beams destroy each other, and a *black spot* is observed at the point of intersection. If either of the beams be intercepted, the spot becomes luminous; but if both be allowed to fall upon it together, it becomes black: either of the two will illuminate it—both together produce darkness.—Now the Copuscular theory fails to account for this phenomenon; but it is clearly explained by the theory of an undulating medium. I can only give the explanation in a general way as thus: It is necessary to show that it is possible for two systems of waves to obliterate each other, in order to the explanation; for if the presence of waves is essential to illumination, any thing which destroys them must produce darkness.—Now if we suppose two systems of waves propagated along the surface of a pond, we may easily imagine that the crests of one system shall fall directly in the hollows of the other—and we should thus have a surface perfectly smooth. This is what happens in this case. The two beams, every time they come together, cause two systems of waves, of which the crests of one fall into the hollows of the other, and the æther is in the same state as if there were no waves at all. Of course, according to this theory, there can be no light.—But if we make the slightest change in the beams, so that the crests of one system of waves shall be out of the hollows, the spot will be instantly illuminated.

But the points which I have been most especially requested to explain is the method whereby the vast velocity of the retina in its vibration has been computed; for that millions of millions of vibrations, performed in a single second, should be a matter of arithmetical computation, cannot be otherwise than astonishing, and may well excite a rational curiosity as to the means by which it is accomplished. I can do no more than give you an outline of the method by which NEWTON established as a matter of demonstration, the number of vibrations caused per second upon the retina by the waves of light.



By referring to this figure, if we suppose the *d i* to represent a plane glass resting upon the slightly convex lens represented by the arc *f g a h*, *a* being the point of contact, in the first place it will be readily seen that, knowing the radius of the circle of which the arc *e a h* is part, and knowing likewise the distance *a b*, *a c*, *a d*, &c, the length of the lines *b g*, *c f*, *d e*, &c, is a matter of easy trigonometrical calculation; so that although the distances *b g*, *c f*, &c, from the surface of the

convex lens to that of the plane glass are too small for measurement, they may still be computed with the utmost degree of accuracy. Now NEWTON did this:—having arranged his apparatus as represented in the figure, he let fall precisely upon the point of contact a beam of light—of pure red light. (You are doubtless all aware that the white light of the sun is composed of light of seven different colors—or as others maintain of a greater number; but in either case the several colors composing white light are called *pure* colors.) Immediately a certain effect took place: exactly at the point of contact there was visible an intensely black spot; outside of and surrounding this was a ring of red light; outside of this was a ring of black—then one of red, &c. He had then a succession of rings, black being in the centre, alternately black and red. The red light which fell upon the point of contact, was reflected and came to his eye in some points while in others it passed through and left a black spot or ring.

NEWTON at once saw that the cause of this phenomenon was this: wherever a red ring was visible a complete pulsation of the ætherial fluid existed; a single wave was propagated between the two surfaces. But in an interval less than this, the wave was broken; the interval was greater than the width of one wave, and less than the width of two, and no light was reflected; consequently a black ring was seen. The next ring was seen at that point where the interval between the surface of the convex lens and plane glass was equal to the width of two waves; the light was consequently reflected and a red ring was seen. Next came an interval greater than the width of two waves and less than that of three; consequently a black ring was seen. The distances between the two surfaces then, represented by the lines *b g c f* and *d e*, supposed to be infinitely small, correspond to the width respectively of one, two and three waves; and at these points red rings will be seen. If then the length of these lines *b g*, *c f* and *d e* can be ascertained, we shall know the width of a wave of light. But as I showed just now, these lines are easily computed, knowing the radius of the convex lens and the lines *a b*, *a c* and *a d*. Thus NEWTON obtained the actual width of a wave of light—(though he used a different name, in accordance with his theory.)

Having arrived at this result with red light, he tried it with all the other colors of the spectrum, and the number of waves in a single inch for each color he ascertained to be as follows: I use round numbers, although he computed it with extreme accuracy.

Red light required.....	40,000	waves in an inch.
Yellow.....	44,000	"
Green.....	47,000	"
Blue.....	51,000	"
Indigo.....	54,000	"
Violet.....	57,000	"

Now I have been asked by many gentlemen, who have done me the honor to attend these lectures, how it is possible to estimate the number of vibrations of the retina for each color. Nothing in the whole range of physical inquiry can be more simple. Hear the method: It appears from what I have stated that Newton ascertained the number of waves for each pulsation in a single inch. Imagine a surface covered with undulating waves so extremely small as these, and you will have a notion of the waves of light. Now it is known that light moves at the rate of 200,000 miles in a second of time. During every second, then, a beam of light 200,000 miles in length enters the eye. And as has just been shown, for every inch of that beam there are 40,000 waves or pulsations for red light, and a certain known number for the other colors. And now can you not see how we are able to determine the number of vibrations on the retina? All that is necessary is to find how many inches there are in the beam which enters the eye in a single second; multiply that number by the number of waves in a single inch for each color, and you have at once the number of vibrations. (Loud applause.)

I confess that at first I shrunk from introducing this demonstration, through fear that I could not make it lucid; but I considered myself *challenged* to attempt it, and now I trust you understand it.

The Lecturer thus closed this beautiful explanation amid the deafening plaudits of the large and highly gratified audience.

LECTURE VII.

VOLTAIC BATTERY.

LADIES AND GENTLEMEN: If we look at the progress of the human mind, both social and political, we shall find that it will be generally marked by a certain steady and uniform rate of advancement. But we shall also notice this: that Epochs occur at certain periods at which it appears to make a sudden start in advance as if acted upon by some temporary and powerful cause. These effects are conspicuous to every observer in the social and political world: we call those in politics by the name of revolutions. But no doubt they arise from causes which are latent and unobserved, and do not depend altogether upon human will or human agency. The phenomena of intellectual advancement are not so apparent, but they are nevertheless subject to nearly the same succession of changes, and are likewise observable by the attentive eye.

The great and sudden changes alluded to as often occurring in the political and social state of our species, are also to be seen in intellectual matters. Every properly informed person is now doubtless aware of the violent and sudden changes which marked the state of the human mind at the close of the last century. Revolution followed revolution; and the mind in its social relations seemed to break down all the common barriers which had formerly restrained it. The splendor of kingly power was darkened both upon this and the other side of the Atlantic, and the human mind appeared to start forward as if suddenly unchained from the thraldom under which it had formerly crouched. This, to an ordinary eye, was certainly confined to social and political matters; but to the eye of the philosopher it was far otherwise, and spread its influence over the whole face of physical science. Its fruits appeared at the close of the last century, and the very first day this was signalized by the discovery of these others. Few weeks had scarcely rolled over before the greatest instrument of physical investigation and inquiry ever produced by philosophy—the *Voltaic pile*—was discovered; and that ex-

traordinary instrument soon, very soon indeed, began in the hands of philosophers to exhibit the most extraordinary results.

Electricity itself is a subject exclusively of modern discovery. For although the ancients were acquainted with a few isolated phenomena, as the attraction of rubbed amber for light svbstances, and one or two other facts which lay barren, unrelated and unfruitful upon the surface of human knowledge, it was in fact not until the time of Franklin that the scattered facts of electricity were collected into one regular form and received the soul and body of a science. This extraordinary agent, as evolved into the machines which were then employed, was produced, not certainly in the power and volume that are evident in the great natural phenomena of atmospheric electricity, but still in too powerful a manner to be examined with that delicate and minute scrutiny by which alone its character could be obtained, and which science demands. In the electrical machine, as you all know, the electricity is evolved by the friction of the rubber upon a glass cylinder; and is thrown forth and collected by apparatus prepared for the purpose, as the Leyden phial, &c. But these receivers were thus charged in inconsiderably large quantities, and when the fluid escaped it rushed forth like a torrent whose restraints are broken down; and we could no more submit this torrent of electric fluid to close nspection than we could the Falls of Niagara to close chemical analysis. Philosophy wanted it in small quantities—in a more controllable and convenient form, so that it might oversee and ponder upon it. Here it was only to be procured in large reservoirs—for the Leyden phial and other apparatus by which it is accumulated can be regarded in no other light than as large reservoirs in which the fluid was dammed up and in a state of quiescence; and if you attempted to pierce the dam and cut a passage for it, instead of flowing forth in a regular and moderate manner, it broke all bounds and rushed out with fury and power—produeing the electric shock and similar well known phenomena.

It remained to discover some way of evolving the electric fluid so that it could be drained out by small degrees—poured forth with regularity and in a form which should allow it to be carefully watched and scrupulously examined. Such a method was discovered by VOLTA in the first month of the present century. He found that, by an accumulation of certain metallic plates, and by the interposition of a certain liquid, not a sudden torrent of the electric fluid, but a gentle and regular stream would be produced subject to his control.

He did more; he provided means whereby he could produce, as he pleased, a greater or less quantity of the fluid—which should be powerful or feeble, according to the circumstances of the metallic plates, which were acted upon in a mysterious manner, which both VOLTA and his successors have failed to explain.

This precious instrument of scientific investigation, which has proved most fruitful and powerful as a cause of progress in our own time, was sent to England in the first month of the present century by VOLTA, accompanied by a scanty and meagre account, transmitted, judging from the style, in an imperfect form, through fear that he should be anticipated in the discovery, and thus robbed of its honor. It fell into the hands of Messrs. CARLISLE and NICHOLSON, the former so well known as Sir ANTHONY CARLISLE, and the latter familiar to all scientific men, in connection with a celebrated philosophical journal. The first application they made of it led to the discovery of the decomposition of water; and as I have already, in a former Lecture, dwelt upon the manner in which this was effected, I shall not now retrace my steps.

After this phenomenon had become familiar, others were observed, apparently of a more extraordinary, but in fact of a less important character. The physiological effect of the instrument was one of the first to excite attention; and surely, if the attention of philosophers was diverted in a great degree by this from the more important phenomena, it was a natural and excusable error, for most astonishing were some of the effects which it exhibited. It was found that the nerves of animals were apparatus of a highly electroscopic character—far more sensible than the finest and most delicate instrument known to Science. It was found that, if the negative pole of the pile were placed under the tongue and the positive above it, (as may easily be done by attaching to the extremities a flat piece of metal,) a certain *acid* flavor was perceived. If, on the other hand, the positive pole be placed below and the negative above the tongue, an *alkaline* taste was perceived.—Again: it was found that, if one pole of the electric battery was applied to any point of the face, as to the cheek, nose, chin, or to any point of that part of the body known to anatomists to be so full of nerves, and the other pole held in the hand, so that a certain portion of the fluid should be compelled to pass through the nerves of the face, even though the eyes were shut, a vivid flash of light would be perceived. This was an extraordinary physical fact; but the most extraordinary of all still remained behind. When the poles were applied to an inanimate body—any organized human body—from which the soul had passed away, it was found that, by a proper application of these extraordinary agents, to various localities in the nervous system, the dead body was made to imitate the functions of life with terrific and revolting precision. The arms were raised and blows struck; the legs extended themselves and kicked with violence; weights were raised far greater than could be moved by the action of the will; the eyes rolled, and even the lungs heaved as if in the act of respiration. Now all these facts have been known for over half a century, with no satisfactory or conclusive explanation—isolated and unfruitful. To say that the nerves possess a high electric susceptibility is nothing more than to state in sounding terms the facts I have expressed in common language. The same agency applied to the various classes of lower animals exhibited very wonderful results. The glow-worm shone with increased lustre; when sent through the nerves of a grass-hopper, the animal chirped with increased loudness. All these experiments plainly showed that there existed some wonderful relation between the agency of electricity and the proximate principle of vitality. What this relation may be—whether electricity itself be that proximate principle or not—has long

been and still remains a subject of active and anxious inquiry.

It was not to be expected that, at the time of which I have been speaking, these results should be exhibited without exciting at Paris—which was then the scientific capital of Europe, and which now London only rivals—especial notice and profound curiosity in high places. Napoleon, who was ever the patron of science and of every art by which its advancement might be aided, summoned VOLTA to the French capital. The particulars of his visit have heretofore been narrated, and I only refer to the matter now to note a circumstance I then omitted. Napoleon immediately proposed a prize of 3,000 francs for the best essay on Electricity, and another of 6,000 francs for any discovery which could be placed in juxtaposition with that of FRANKLIN. This last remained long unclaimed; but after a season it was justly, fairly and most liberally awarded to Sir HUMPHREY DAVY; an act of liberality, coming as it did from a nation then at war with England, and which the English had been taught to consider their natural enemy, unparalleled in history.

But I am anticipating somewhat, in point of time, the matters to which I wish to call your attention. At the very dawn of the life of this science, a narration of which I have endeavored to give you, there existed in the obscure village of Cornwall an individual, a young man, employed in the capacity of a clerk, or some such occupation. There happened to be at Bristol a physician engaged in establishing some baths, who wanted an assistant who should have some little knowledge of chemistry, and who could be depended on for general accuracy and candor; and this Cornish young man, whom I mentioned, fortunately for science and the advancement of the human race, found a place with this physician. Soon afterward, the Royal Institute of London wanted a superintendent for their chemical laboratory. The physician's clerk had contributed some trifling scientific articles to the periodicals of the day, which had attracted the attention of Count RUMFORD, who was incited by them to procure for their author the vacant situation in the Institute. He came to London, and just at this period the Voltaic pile was brought to that great city. The new superintendent of the laboratory was Sir HUMPHREY DAVY, and the moment he became acquainted with the power of the pile, as shown by CARLISLE and NICHOLSON, and as shown with greater clearness in the Institute at Paris, he saw that here was a field of distinction, and he at once determined that it should not lie uncultivated. The decomposing power of the pile on water naturally attracted great attention. Although I have already mentioned the subject, I will take the liberty again to call your attention to the manner in which the Voltaic pile made known the decomposition of water. (Let me remind those not familiar with the elements of chemistry, that water is composed of two gaseous substances, oxygen and hydrogen.)

The manner in which the decomposition of water by the Voltaic pile was established, was this: In a vessel of water were placed at some distance from each other two wires, one connected with the positive and the other with the negative pole of the Voltaic battery. Now mark the effect of this arrangement. It was found that, the wires being of platinum, with which oxygen will not form a combination, bubbles of oxygen gas arose from one of the wires in a rapid stream, and bubbles of hydrogen from the other. That these were the two gases evolved could easily be proved by catching them in a glass receiver, and submitting them to the usual chemical tests. It was found that oxygen gas issued from the positive wire and hydrogen from the negative; and on comparing the two they were found to be evolved in just the proportion necessary to the production of water. But more than this—it was found that if the vessel was weighed after the gases had been evolved, it would be found to have lost a weight just equal to the two gases. Thus there was no doubt that the two gases were produced from the ends of the wires. But a mysterious circumstance still remained, and, I may say, remains to this day, unexplained. In what possible manner—by what conceivable means could one of the two gases, which were continually evolved at the end of one wire, escape to the extremity of the other? for if we suppose a particle of water to be decomposed at the extremity of the positive wire, from that particle we shall have a particle of oxygen, which shall rise into the air. But where is the hydrogen? If you say, "O, it travels, of course, across through the water to the end of the other wire," the answer is, that no such gas—no bubble of it is seen, as it would be if it actually traveled through. Again: if you take a particle at the end of the negative wire, and suppose it decomposed, it must of course give a particle of oxygen and one of hydrogen gas: the hydrogen gas is seen to rise and escape. But what becomes of the oxygen? It cannot travel across, but it is not seen. Here there is a difficulty of a most inscrutable nature. But what is still more extraordinary, if you suppose a partition to be constructed through the middle of the vessel so as to divide the water into two parts, which shall have no connection with each other except by a conducting wire, passing from one to the other, still the same effects will be observed. The bubble of oxygen will be found to escape from the positive wire, and hydrogen from the negative.

Now soon after this singular effect was observed, many theories were devised to account for it; and among them was that of a continental philosopher named GROTTHUS, which is so ingenious and beautiful that I shall not hesitate to give a sketch of it, although I shall be afterward under the necessity of knocking it down.

GROTTHUS maintained that the elementary particles of water were naturally charged with positive and negative electricity. Take a single molecule: it is composed of two parts, hydrogen and oxygen: the two are held together by the mutual attraction of the two opposite electricities which the particle contains. Now, says GROTTHUS, things happen in this way: when the stream of positive electricity is sent to the point of the wire, it attracts the negative electricity of the particle nearest to it, to itself. And as the positive electricity of the oxygen has an attraction for the negative electricity of the wire, the particles turns its oxygen side to the wire, and the hydrogen being repelled by the positive fluid is turned away from the wire. Therefore, said he, the molecule shifts its position until it gets its oxygen face to

the wire, and its hydrogen face from it. Now, this particle having thus changed its position, the negative electricity of the hydrogen, which is turned from the wire, attracts the positive of the molecule next to it, which likewise shifts its position until its oxygen face is turned toward the first particle, and its hydrogen from it; and the same thing goes on through the water, so that you will have a row of particles from the point of one wire to the point of the other, ranged along between the two: the oxygen forces of the particles being all turned toward the positive wire and all the hydrogen faces towards the negative wire.—Now, said Grotthus, what happened? you say it is very extraordinary you see no bubble; not at all extraordinary, for the positive wire lays hold, by the strength of its attraction, of the oxygen of the nearest particle and discharges the hydrogen: but that hydrogen does not ascend, because it is seized by the oxygen of the adjacent particle of water; the discharged hydrogen again lays hold of the oxygen of its neighbor and thus it goes on, by a successive action, from one particle to another until at length it reaches the last particle of water which has its oxygen seized by the hydrogen of the adjacent particle and the hydrogen rises. This accounts for the fact that you see no bubble; because the rapid succession of decompositions, and re-compositions of the successive particles of water, causes the hydrogen to pass from one wire to the other until it is finally discharged. This is an extremely pleasing and ingenious theory: but it seems to me to fail entirely at this point; you recollect that I showed that when a partition is passed through the vessel so that the water is divided, the same thing takes place, so that here the successive decompositions and recompositions of Grotthus won't do, unless you suppose that the particles of solid wire undergo the same change as the particles of water, which will hardly be admitted. This theory never received the general assent of the scientific world, and I may say that the facts I have detailed are still entirely unaccounted for.

When these facts of the analysis of water began to attract attention, Davy commenced his experiments, and he found that whatever the principle which decomposed the water might be, the liquid, after the decomposition was effected, was found to be strongly impregnated with acid. Various theories were devised to account for this, some maintaining that the atmosphere was the source of the acid, and others going the extreme length of believing that the acid was produced by the action of the electricity; and on this theory they gave to it the name of *electric acid*. In this state of confusion the matter lay when Davy took it into hand, and determined to seek out the cause of this phenomenon; and nobly did he succeed.

The task of philosophers is not always the grateful one of evolving new and important truths; they often have the more difficult and thankless labor of extirpating error; and such was the task of Davy in the inquiry upon which he had now entered. He found by the closest and most careful investigations that the production of acid in the water, when exposed to the action of the galvanic current, was an error: but the process by which he arrived at this conclusion is too interesting to be passed in silence. Proceeding with the sagacity which enables one to seize upon the accidental circumstances which often arise out of an investigation, he commenced his labors in this way. He determined to perform the experiment most carefully—in a manner which he thought could leave no possible chance for the existence of acid from external and unforeseen causes. He at first supposed that the acid arose from matter used as a conductor, and for this Wollaston suggested as a remedy, that a fibrous substance, as asbestos, should be employed. Davy took two agate cups, poured into them distilled water, and employed as a conductor a skein or syphon of amianthus. He then connected the water in the two vessels with the two poles of the galvanic battery. As usual, and as he expected, bubbles of oxygen came from one and hydrogen from the other. After the process had been going on for a little time, Davy applied the usual tests, and, contrary to his anticipations and greatly to his disappointment, he found that there was acid in the positive vessel and alkali in the negative. He felt sure that they could not have existed in the water, nor could he admit that they came in through the agency of electricity.

Well, he began to consider, may it not be that the *cups* themselves are the source of this principle; and to test this, he proceeded in this way: he got made two cups of fine gold in the form of hollow cones, carefully executed. In these he put the same quantity and kind of water as before, subjecting it for the same time to the same battery and under the same circumstances, and now, said he, if acid is produced in any different quantity, I am certain it cannot be attributed to the water or the electricity, for both are the same as before; but if, as I shrewdly suspect, the cups have any thing to do with it, it will become evident, and I shall either find no acid at all, or a different quantity from what I had before. The result completely fulfilled his anticipations, not by the total absence of the acid and alkali, but by their being found in very different proportions. There was little or no acid in the positive cup, and twenty times as much alkali in the negative, as in his former experiment. He had no doubt now that he had arrived at a method which would finally lead to the discovery of the cause of these phenomena; but he determined to try a still further experiment.

He accordingly put into a silver still a quantity of the water he had before used, and distilled it at the low temperature of 140° by a slow process; after having distilled it until it had all passed off in vapor, he examined the residuum and found that it contained seven-tenths of a grain of alkali.—Here, then, he had found the source of the alkali; it was contained in the water with which the experiment was made. After this he repeated the experiment with the same quantity of water exposed to the same galvanic action and in the same gold cups; and the result showed conclusively that the belief that the electricity itself was the source of the acid and alkali, was completely exploded and an error removed from his path which would otherwise have stood in his way in all his future discoveries.

The next subject which attracted the attention of Davy, and in examining which he arrived at the very important discovery which gained the

prize offered by the French Academy of Science, was an investigation of the manner in which the elements of matter behaved themselves with reference to each other while under the influence of the electric current. You will comprehend this more clearly when I show you the experiments which Davy made and their first most curious and interesting results.

He took two vessels, filled each with water, and caused them to communicate, the one with the positive and the other with the negative pole of the galvanic pile. Between the two he placed a third vessel, likewise containing water, which was connected by syphons with the water in the other vessels. In this arrangement the stream of positive electricity passes into one vessel, through the water in it, through the connecting wire to the middle vessel, thence by a like conductor into the third vessel, when it meets the negative electricity from the other pole of the battery. Now his first experiment was this: In the middle vessel he placed a saline solution, which is a compound of an acid and an alkali. In the other two vessels he placed pure water. The moment the electric stream was allowed to enter the vessel a singular effect took place; the salt held in solution in the middle vessel was immediately found to be in process of decomposition, the acid constantly traveling over into the positive water and the alkali into the negative. Now here he saw indications of a law; it was quite plain that the positive electricity exerted a power of attraction on the acid and the negative on the alkali.

The next step Davy took was this: He said, if the attraction of electricity for those fluids is so powerful, may it not happen that, by putting the solution of salt into the positive vessel, while pure water remains in the other, the negative electricity in the pure water will draw over the alkali from salt in the positive vessel, leaving the acid where it was. He accordingly performed the experiment, and found the result to correspond precisely with his expectations. The salt in the positive vessel was decomposed, its constituent parts were torn asunder, and the acid continued faithful to the positive fluid, while the alkali traveled over to the other vessel and deposited itself as close as it could possibly do to the negative rim. To leave nothing undone, Davy varied the experiment by by placing salt in the negative vessel, and found that the same principle was as active as before.

He remarked that the water in the middle vessel remained pure, although all the acid in the one vessel, and the alkali in the other, had passed through it. This was sufficiently remarkable; and he saw that something more remained to be examined. He considered that the water in the middle vessel must be in a peculiar physical state. Was the mass of the liquid through which the acid and alkali were in the act of passing free from either, or would it have the qualities of a solution of either of these principles? If it would not, it would certainly be a most remarkable fact. To decide this, he made the following experiment: He passed the electric fluid through the vessels arranged as before—the alkali of the salt in the outer vessel passed through the liquid in the middle vessel to the other extremity. Now to determine whether the water in the vessel contained alkali, he applied the usual tests, and found that there was not the slightest indication of its presence. He went farther—he reversed the experiment, and made the acid pass through the middle vessel; but the water gave no note of the presence of acid. He applied the most delicate tests, but could get no indication of any acid in the middle vessel. The effect which would instantly be produced if there were no electrical agency in this experiment, was totally lacking in presence of this magical influence.

Now you 'll ask for the reason of these remarkable facts. Can they not be accounted for—is there no way to explain them? There is not; no philosopher, so far as I know, has ever arrived at any thing more than the naked fact. All that is known is this, that when natural bodies are under the influence of electricity, they behave themselves very differently from what they do in their sober senses.

Nothing can be more felicitous than that philosophical instinct which enables observers to avail themselves of the accidental circumstances which arise in the course of their inquiries. Every one who has ever engaged in philosophical investigations must have experieneed that when he has been hunting after some fancied truth, accidental circumstances have pressed upon his attention, which, if duly considered, would be seen to be of infinitely greater importance than the truth of which he was in search. This occurred in the case of Davy, in the investigation to which I have made allusion; and nobly did he profit by these accidental hints—or I would rather regard them as promptings of Him whose will it is that knowledge should progress.

I told you that when Davy observed the circumstances attending the decomposition of water by the action of the galvanic wires, he found an acid evolved in the agate cup. When gold was substituted instead of agate, little or no acid was evolved. Said he, 'O, I see the cause of this: the acid comes from the decomposition of the agate cup.' Thus would any one have said, and an ordinary mind would have passed it by as an accidental circumstance of no moment. But Davy saw in this accident the germ of probably the greatest discovery of modern times. 'If,' said he, 'the power of the electric current be so irresistible, so omnipotent, as to tear asunder the elements of agate, what effect may we not anticipate if we apply this to substances which have never yet yielded to the analytical power?' The idea took possession of his mind—that the attraction of the positive and negative poles of the battery for certain elements of bodies, that which took the acid and alkali to the further vessel in his experiment, was the same powerful influence that decomposed the elements of agate; and he never let his mind rest until he had brought it to bear upon this point. Accordingly, in his first experiment, using cups of various metals, he found he had a most powerful agent to play with. After much pondering and deep reflection upon this, he arrived at this conclusion—that substances classed as alkaline and earthy, in all probability, are not what they appear. Many of them, if not all, which had previously been deemed simple substances, he had a deeply rooted conviction might be decomposed if he could bring to bear upon them

the poles of a sufficiently powerful Voltaic apparatus to tear their elements asunder. The first substance which he submitted to experiment, with a view to this belief, was *potash;* and there is recorded no more elegant and lucid experiment, nor have any results been more important. He took a small quantity of potash, and placed it upon a platinum spatula or disc. He wished now to transmit the electric current through it, and for that purpose placed the positive wire at one side and the negative at the other, expecting that its elements would yield to the opposite force and be decomposed.

But in its dry state it would not pass, nor would the current be transmitted unless aids could be procured to assist in conducting power. The first suggestion was to dissolve it in the water. He did this, and submitted the solution to the action of the Voltaic pile; but he found that this most wayward agent had preferences just as bodies had axes of preference around which they would revolve. The electricity would seize upon some substances in preference to others when two or more were present, nor would it attend to more than one at once. And when it was transmitted through the solution, the thing it decomposed was the water and not the potash; and the only effect of the Galvanic action was to decompose a portion of the water, and thus leave the solution of potash stronger than before. Davy next attempted to render the potash a fluid, since any liquid, he well knew, would transmit the current more readily than a solid. He procured a platinum spoon, put into it a small quantity of pure potash, and exposed it to the flame of a lamp blown upon by oxygen gas. When the potash was melted, he put the opposite sides of the spoon in contact with the opposite poles of a battery. The instant a communication was formed, when the metal spoon was positive and the wire inserted in the liquid was negative, a vivid, sparkling flame arose from the wire, and from the spoon issued a gaseous substance and escaped into the air. Davy at once supposed that the potash had been decomposed, but that the constituent part which escaped at the negative pole was so combustible that at the heat required it immediately took fire.

This conclusion was perfectly correct; but he had yet failed to seize and submit the element to examination. Another experiment was still to be tried. His final experiment was this: He took a small quantity of potash, and by exposing it to the moisture of the atmosphere, he got a coating of moisture upon its surface. This gave to it a sufficient conducting power, and he again placed it upon the platinum disc in communication with the negative poles of a Voltaic battery, while upon the surface of the potash he applied the positive wire, and what was the result? This: *At the upper surface of the positive pole, there were emitted bubbles of gas; while on the platinum were deposited metallic globules having the appearance of quicksilver.* These soon acquired a film which protected them from the atmosphere and preserved them in their metallic state, so that they could be submitted to examination. It was found that the gas emitted was pure oxygen; the discovery was complete, and the important fact was established that potash, theretofore supposed to be simple, was a combination of pure oxygen and a metal not before known, and that the two were separated by the attraction of electricity—the metal by the negative and the oxygen by the positive. The metal deposited was the base of the alkali, since called *Potassium.* Soda was decomposed in the same way and with a similar result, giving Sodium; the same process with the alkaline earths led to the discovery of Barium, Strontium, Magnesium, and Calcium, as the consequences of this great discovery. It was thus shown that *all the earths and all the alkalies are compounds, composed of oxygen gas and various metals.* Thus the number of simple substances was greatly lessened, while the catalogue of metals was increased. I am sure that I do not overrate the importance of this as a scientific discovery, when I say that it is by far the greatest of the present century.

Having thus concluded his Seventh Lecture, Dr. LARDNER exhibited his fine moving diorama of the heavens, at the same time remarking upon the different celestial appearances—as he had done on former evenings. He also made some observations upon the double stars, which are supposed to be twin suns for twin systems. These are often of complementary colors, as red and green, and some of the speculations of Sir JOHN HERSCHELL, the most poetic of astronomers, upon the vicissitudes of day and night upon the planets which are lighted by these suns, are exceedingly curious, and not without some foundation. Thus, a planet, while it was in that part of its orbit between the two suns—one of which was red and the other green—would be shone upon by both, and would have a red day and a green night. When one quarter of its orbit from that position, the planet would have the first part of its day green, and the the rest red; and so the curious vicissitudes of day and night might be traced for every portion of its orbit.

LECTURE VIII.

STEAM ENGINES OF GREAT BRITAIN AND AMERICA.

THE closing Lecture of the Series was mainly a description, by reference to models, of the Steam Engines of Great Britain and America as applied to water navigation and to railway transportation. Depending, as it did to some considerable extent, upon the beautiful models by which it was illustrated, for its interest, it would scarcely be intelligible in a literal report, unless figures could be presented which should serve instead of the didactic models; this we cannot conveniently do, and shall throw our report, therefore, somewhat into the form of a notice, collecting from the lecture such remarks as concern the general principles which Dr. LARDNER attempted to elucidate, and omitting what could be of no possible utility without the explanatory models.

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He commenced by stating that the arrangements required by the application of steam engines to purposes of sea voyages, are different altogether from those required by engines which are to work on land, or which are intended for river navigation. For sea navigation it is generally admitted to be essential that the enginery should be between decks. This is especially the case with vessels intended for the National Marine. It is likewise

evidently necessary that the engine, or the boiler, should be fed with salt water. These are the two features which enter into the construction of vessels built for sea navigation, which have no application upon rivers or railways. When steam vessels were first employed for coast navigation the difficulty attending the first of these conditions was obviated, as I explained on a former evening, by transposing certain portions of the machinery —by transferring the beam to the bottom instead of suffering it to remain at the top—and by having the connecting rods pass upward instead of downward, so that a part of the engine was, in fact, turned upside down. The proportions of the cylinder were altered to accommodate these views: the cylinders of engines are generally of greater length than width, being commonly twice as long as their diameter.

When I mentioned this subject before, I made an observation which I now repeat to be my strong opinion—an opinion about which I have no doubt whatever that the profound reverence which is entertained by the English engineers for the authority of WATT, did *harm* to the progress of steam navigation, and especially to that branch of it applied to sea purposes. All of the present audience who have happened to read the works I have written upon this subject, and the scattered memoirs I have myself penned with respect to Mr. WATT, will readily acquit me of any intention to speak or to infer any thing disrespectful to that great man. But truth should be far above all personal considerations, and the plain matter of fact is this: Mr. WATT, the inventor of the low pressure condensing steam engine, had a natural and excusable bias in its favor; and during his lifetime he could hardly listen to the name of a high pressure engine without irritated feelings; and these feelings of irritation seem to have descended upon his successors. So far has their aversion to high pressure engines been carried that for many years after their general use none were were allowed to be built at Soho; and this particular form of the steam engine, which has made its efficacy felt to the outskirts of human civilization, the consequences of which have spread to the most savage tribes of Africa and Asia, and brought the blessings of internal communication, at a very early period, to this vast continent, was despised and utterly rejected. These feelings spread among the English people to such an extent that it was perfectly vain at any time to propose high pressure engines for English steam vessels; and had it been done and a vessel actually furnished with one of them, the consequences would have been that no one would have undertaken a voyage in her. At the first construction of engines for the English railways, it was seen that they could not be worked, not only without high pressure engines, but without engines of the very highest power; and the sagacious engineers, well knowing the prejudice against them, wisely adopted for them the name of *locomotives;* and had they been called, as they were, high pressure engines, it is likely that the success of railways would have been greatly less than it was. But they were christened locomotives; and I verily believe that to this day not one in twenty thousand of the English people are aware that a locomotive is a high pressure engine. I say, that it is my strong conviction that the reverence for the name of WATT greatly retarded the progress of English steam navigation. In the construction of their marine engines the English adhered as closely as circumstances rendered possible to the old model—some little changes, which were seen to be perfectly inevitable, being made.

The second condition necessary to engines used at sea, as I stated, is that the boiler be fed by salt or sea water. Now sea water is a solution of certain salts; the water however is not *saturated*, that is, it does not contain as much salt as it is capable of sustaining. If it be put into a boiler, then and evaporated, the water passes off in steam while salt will remain, and there will be a constant accumulation of salt water in the boiler. The effect is that the water in the boiler becomes a stronger and stronger solution of salt. If this goes on for any considerable length of time, the water in the boiler will become completely saturated; and in this state, the next step will be a precipitation of salt in in the boiler; this will be seen to be the necessary consequence; for there being more salt than the water will hold in solution, there is no alternative but to throw it down in the boiler and if the process be carried on, after a time the boiler, will in fact be converted into a *salt pan.*

These evils, even if not carried to the extreme limit I have supposed, will still be considerable, as an incrustation will be formed upon the inside of the boiler of salts and earthy matter which are found to have so strong an affinity for the metals that they will adhere to the surface. This will continue to increase until the boiler will be walled up by a thich coating of earthy matter. This will be attended with important and injurious consequences; for the boiler being thus made a nonconducter of heat, places the metal between the fire and the internal coating which would not allow the heat to pass beyond it. Thus the heat was accumulated in the metal and often caused pressure in or bursting of the boiler. But even supposing these extreme consequences not to be experienced; still even the thin coating which would be formed shuts off and thus destroys a portion of the heat. This soon became apparent, and various remedies were suggested. One of the most obvious methods of remedying it, was this: As the water became saturated, an aperture was made in the bottom of the boiler to let off the *brine*, which would be forced out by the pressure of the steam inside the boiler; when this was out more sea water was introduced in its stead. Now the desired effect was easily produced by this arrangement; for the brine, being heavier than the water, would naturally fall to the bottom and pass through the aperture. This process was called *blowing out*, and great attention was paid to it by the Government Engineers.

But it was found that a difficulty was to be encountered in cleaning the boiler by this process, and that this in fact was but an expedient which involved an evil of great consequence, especially on long sea-voyages, when fuel is of great value, and when it is a *desideratum* to procure the greatest possible amount of steam with the least expense of fuel. The brine which was blown out, it will be recollected, was of the same temperature with the water, and in blowing the brine out and receiving cold water in its stead, a portion of the

fuel was consumed to no purpose; and this, with reference to long sea-voyages, was an evil of serious magnitude. One of the first expedients suggested as a remedy was extremely ingenious, yet not novel, although the inventor was probably not aware that it had ever been suggested before. The process of feeding the boiler with salt water was proposed to be abandoned, and it was to be filled in the first place with fresh water, and it was also proposed to carry the steam, after it had worked the engine, into a condenser of peculiar construction. Some hundred tubes were passed through a cylinder of cold water: and the steam was suffered to pass into these tubes so that it might be condensed not by mixing with cold water as in ordinary condensers, but by the cold surface of the pipes. This contrivance was miscalled HALL'S condenser, for it was really invented by WATT, and at an early period he laid a drawing of it before the House of Commons; but its use was abandoned then for the same reasons which led to its rejection since. It was found that the steam, in passing through the pipes, was not condensed with sufficient rapidity—although its final destruction was certain. But it will be seen that if, after the piston has been driven to the top of the cylinder, steam be not instantly destroyed below it, it will encounter a resistance on its return, and thus the power of the engine will be diminished. The destruction, then, of the steam is not of more consequence than its *timely* destruction. I believe that for the sake of the experiment this kind of condenser was employed on the British Queen: but it is now universally abandoned.

This method then would not do; and the most effectual method of keeping the boiler clear since employed, is that by the use of what is called FIELD'S *brine pump*. There were two objections to the system of 'blowing out;' one, that it required an attention from the engineer which could not always be obtained, and the other that a quantity of heat was lost by every discharge of the brine. FIELD'S brine pump, with which I am acquainted only by information, seems to meet both these objections. Two things are required of it; first, that it should pump out a proper quantity of brine; second, that it should pump into the boiler a proper quantity of sea water; it was necessary, in the third place, that little or no heat should be lost. Now FIELD calculated the proportion which ought to exist between the water to be pumped in, and the brine to be pumped out; the difference is clearly the quantity which works the engine, and represents its power. Having calculated the necessary proportion of these two pumps, FIELD contrived this elegant apparatus. The feed pump was a tube through which the sea water was driven; and within that tube was another of the proper size through which the hot brine was driven out. Here then, within the outer tube were two streams of water; the hot brine passing within and opposite to the stream of water which was forced into the boiler. Why, then, what happens? The heat contained in the brine is constantly communicated to the cold water which passes in, so that none goes into the boiler at a lower temperature than 100°, which is nearly as high as any feed could be expected. By this beautiful expedient the heat which was carried off by the brine was carried back by the water.

But the most important feature of this invention is the mathematical certainty with which the proportion of the two pumps is determined. From all that I have been able to collect concerning it, I believe that this expedient will perfectly answer the purpose; and that it will keep the boiler as free from salt as any expedient can be.

These circumstances, perhaps, will sound of less importance in this country than they were felt to be in England; for you appear to be just commencing the construction of sea-going steamers, in which, with the exception of the coast-vessels, until recently but little has been done. The English, however, have a large class of sea-steamers, of which you see the type in the Great Western and the Cunard vessels. This country is about to enter upon the constructien of vessels for the National Marine, and in the first ship constructed, may be seen the free spirit which I have so often mentioned as striking a stranger with the most surprise on first coming to America. More novelties in mechanics and general science have been presented to me in this country within twelve months than I had ever seen in twelve years in England; and this although I have examined but few vessels in this country, while I have seen thousands in the Old World.

Three or four national vessels are to be constructed in this country, and one of them, the Mississippi, built at Philadelphia, so closely resembles the English steam-ships, that, after what I have already said, but few observations are now necessary. This is built closely after the English model, with some little variations of no material importance. The cylinders are nearly square; the beams are placed at the foot of the cylinder, and the boilers are made of copper, which are less liable to be incrusted by the deposition of salt, than those made of iron. The cost of copper boilers in England is almost three times as much as iron; here it is not so great, and as they last twice as long, and in England three times as long as iron, the difference of expense is not material. There is, however, less reason for using copper in this country than in England, from the fact that you possess a quality of iron which, for boiler-plates, is quite superior to English iron; of this I cannot speak from personal experience, as I have not had an opportunity of examining it; but I am given to understand that it is so far superior that boilers of American iron are nearly equal to the English copper boilers. For this reason it would probably be inexpedient to introduce copper boilers to any extent in this country.

Another difference of considerable importance in construction between the Mississippi and the English vessels is in the valves. When the valve-rod in these large steam-engines is an immense iron bar, and the valves themselves large and heavy, it requires the force of several men to start them when the engine is first put in motion. Here *balance-valves* are used, which are moved by the difference of the pressure of steam upon their surfaces, one of which is made purposely larger than the others. They are, therefore, easily started. [The theory of these valves was fully and clearly illustrated by a model.] These balance-valves, so far as I know, have never found their way into England. You may ask—Is there no objection to them? The re-

this objection: that if the rod connecting the two valves be not precisely and permanently of the requisite length, the valves will not accurately fit their seats, and will thus allow a portion of steam to escape. This, however, I am informed is remedied by accuracy of construction and by using different metals for the connecting-rod, which will prevent its expansion by heat, and insure it to be always of the same length.

Having now dismissed the Mississippi, and having on a former evening explained the construction of the engine of which ERICSSON is the inventor, I will now briefly notice the Missouri, a steam frigate now being constructed at this port; and here we see the freedom in the construction of steam engines which I have mentioned as never being witnessed in the English machinery.

I have already said that one primary object in adapting steam engines to purposes of marine service is that the enginery be kept between decks, and to diminish its altitude. We have seen how it is done in the English vessels, by placing the beam at the bottom of the cylinder, and by turning a portion of the machinery upside down; we have seen, too, that in the ERICSSON engine this is accomplished by halving the cylinder and placing the two parts upon their sides. Now, without taking so bold a step as the last, and without adhering slavishly to the English models, the engine in the Missouri is placed in an inclined plane; the cylinder, placed upon wooden supports, is inclined, and the valves employed are balance-valves. One advantage of this construction is, that it is not necessary to cut away the cross-beams of the vessel as much as in the other cases. The bulk of the engine is nearly the same, but the weight and expense are from 10 to 15 per cent. less. As to ERICSSON's engine, it is impossible to form an accurate estimate of the expense, as its construction is not yet complete.

On the evening next succeeding that on which the Ericsson engine formed the subject, I made some remarks upon the peculiar propeller to be used with it; but as, in consequence of the inclemency of the evening, few were present, I will again advert to the subject. The wheel has an external resemblance to the screw, but it has none of its geometrical or mathematical properties.—The first distinction to be noticed is, that this propeller is intended to be sub-aqueous. This will allow it to be worked as a sailing and steam vessel at the same time, which is not the case with those in common use; for the wind in the sail careening the ship to one side, the paddle-wheel on the other will be out of the water. The common paddle-wheel is also subject to great inconvenience from a rough sea, which is entirely obviated in this case.

[That there was no lateral action to be apprehended in this case, involving a loss of power, Dr. Lardner showed by a beautiful experiment, in which the effect of a rapid motion of the wheel upon the air, which answered instead of the water, was exhibited.]

With respect to the other branch of the subject announced for this evening, I shall merely have time to notice a few of the historical circumstances connected with the application of steam engines to railway purposes. You are all aware that the power of the engine depends on the hold which the engine has upon the rail. When the wheel resting upon the rail is turned about, one of two things must evidently take place; either the wheel must slip and the engine remain stationary, or if the wheel has sufficient hold upon the rail, the carriage with its train must move forward. It is curious that a fact so material as that the wheel has a hold upon the rail should have been overlooked by engineers; and that for many years the engine should have been unapplied to railways merely for the supposed want of such a hold. It was supposed to be necessary that, in order to give any power to the wheel, it must have a certain purchase; and as none was seen, they concluded there could be none. It never entered their minds to ascertain by direct experiment whether the wheel had any such purchase. A thousand contrivances were devised to afford some hold for the wheel upon the rail. One was to make teeth in the rail and the wheel, and another person proposed to make a pair of mechanical legs, which should be placed the one before the other, and thus enable the carriage to walk off like any biped.—These are only two out of the various expedients suggested. At last it occurred to some one to try whether a single carriage might not be drawn over the railway by the pressure of the wheel upon the rail; and to his great astonishment, he found that search had been diligently made for many years for that of which there was no need.

When the engines were first applied to the transportation of passengers in England, engineers naturally enough directed their attention to obtaining a high evaporating power, and as their power evidently depended upon the rate at which water could be evaporated, the problem became, how to evolve from a limited furnace as great heat as possible, and how to produce a sufficient draft for the fire, as tall chimneys were not admissible. The expedient resorted to were these: a furnace was placed behind the engine called the fire-place, closed upon every side, and the air which supplied combustion rose between the grate bars. The fuel used on the most rapid railways in England is nothing but the best coke. This is put into a high state of combustion, so active, indeed, that when the door of the fire-place is thrown open the eye can scarcely bear the splendor of the blaze. Now the heated air if it were suffered to pass immediately into the chimney and there escape, would take away with it a large quantity of heat. In order, then, to make it linger in contact with the water until it gives up all the heat which can be extorted from it, this expedient was proposed by Mr. BOOTH, Treasurer of the Liverpool and Manchester Railway, who had, for some time in the early part of his life, been a merchant, and whose knowledge of mechanical science had been wholly self-acquired. In a locomotive used to transport coal, a large metal pipe was carried from the fire-place backward and forward through the boiler and then made to ascend the chimney; the heated air thus had to pass through and return, giving, meantime, its heat to the water. It was soon evident, however, that this was insufficient for the purpose, and a prize of £1,000 or thereabouts was proposed by the British Government for the best method of saving the heat. Several plans were offered and the one adopted was the travers-

ing the boiler with a great number of tubes, which were at first some two or three inches in diameter, but were afterward reduced to from one inch to an inch and a half. These tubes are from 100 to 150 in number and the heat traversing them is imparted to the water.

But it was soon found that by this arrangement the draft was insufficient to keep up an active combustion; and I shall now mention a circumstance which, like a thousand others, shows the important part accident often plays in leading to important discoveries. So far as I have been able to ascertain, the discovery of a most important principle in locomotive enginery is due entirely to chance. It happened in one of the engines used in one of the Northern and cold counties of England, the escape of the waste steam was attended with serious inconvenience; in fact, it became a perfect nuisance, and the engineer knew not how to rid himself of it. It finally occurred to him to send it up the chimney, and thus to let it pass off with the smoke. It was immediately seen that he had here a power of tremendous magnitude; for the moment the steam passed into the chimney—as the engineer might have seen by a little reflection, if he had any, would be the case—it acted like a powerful bellows, producing a draft in exact proportion to the quantity of steam used in the cylinder. The fire was thus regulated by a principle which acted in exact proportion to its wants. This plan was immediately adopted every where, and is now called the *blast pipe*, and on a proper regulation of this depends the efficiency of the engine.

It is quite surprising to see what amount of evaporating power has been obtained by some of the largest engines on the English railways. One on which I had occasion to make some experiments, was found to evaporate 200 cubic feet of water in a single hour. Those conversant with steam will see what heat must be evolved to do this, since to convert water into steam in this way requires a heat three or four hundred degrees hotter than red hot iron.

In English engines, the cylinder is placed under the machinery, and between the two wheels, buried, in fact, in the smoke-box. But in American engines, a different arrangement is adopted; the cylinders, being outside the wheels, are, therefore, easily accessible, and have more space than in the other case. In the English engines, the power is applied by a crank; but in the American, by a pin in one of the spokes.

Although the average speed of the British railways, owing to the number of stoppages and other hindrances, is probably not greater than that in this country, still the rate at which the engines run while in active motion is greater than that known here. They generally go at the rate of from thirty to thirty-five miles an hour, and I have known them, on a regular train, exceed forty. This speed, when the cylinders are outside the wheels, is attended with an inconvenience arising from the lateral motion to which it is liable, causing a continual strain; but when the moving power is brought near the centre this is obviated.

In the English engines there being nothing that corresponds with the perch of a carriage, great difficulty has been experienced in turning short corners; and if this be attempted when the engine is going with great speed, it will be thrown off the track. This gave occasion for a special report in the House of Commons, forbidding the construction of curves upon railways with a radius of less than a mile. The consequence was that great expense would be incurred to avoid these curves. But had the cars been constructed with a perch, there would have been no difficulty in turning corners; and on my arrival in Philadelphia, I saw, what may never be seen in England, a train of cars entering a town and turning a corner with the greatest ease. This is accomplished by a simple contrivance called a truck frame. Your engines too are furnished with small wheels, called *guide wheels*, which are not found in the English, which are only capable of going directly forward.

The speed of your engines is not as great as that of the English, mainly because your wheels, —called *drivers*—are smaller than ours. They are usually about four or five feet in diameter, while on the Great Western Railway, they are from seven to ten. The effect of the size of the wheels upon the speed of the engine is very obvious; for with every revolution of the wheel a space equal to its circumference is passed over.

But I find that this subject, although the time of this evening's lecture has quite expired, is still far from being exhausted; and as this is the last evening of the course, I can only say that, if the same favorable disposition continue to be evinced by those who have attended these lectures, I shall hope to have a future opportunity of discussing this most interesting subject more at length.

THE COMET OF 1843.

[While the foregoing was passing through the press, the following articles appeared in "The New York Tribune," in reference to a Comet, or supposed Comet, which made its appearance about the first of March. These articles, although not originally designed for any thing more than temporary newspaper existence, are not deemed inappropriate to the general scope of the present work.]

'Public opinion' seems to be greatly divided as to the genuineness of the strange visitor which alarms or puzzles our hemisphere just at present. The *Savans* of Yale and the Editors of nearly all the papers who have said any thing of the matter, concur in the belief that it is a *bona fide* Comet, albeit it shows no head—some saying that it has none and others that it is so near the Sun as to sink beneath the horizon before it becomes visible. The Washington papers, however, flatly deny that it has any rightful claims to Cometic dignity—but that it is nothing more than a fine exhibition of Zodiacal Light, of which it gives the following description:—

ZODIACAL LIGHT.

This interesting phenomenon in the heavens was noticed here on Monday night and also on Tuesday and Wednesday nights.

Zodiacal light appears in the morning before sunrise and in the evening after twilight. It is a pyramid, with the sun for its basis. The sides are not straight, but curved, as those of a lens when viewed edgewise. It is generally seen about the period of the equinoxes, when there is the shortest twilight. This light resembles the milky way, a faint twilight or the tail of a comet. The intensity of the light, its shape and tints, may be varied according to the condition of the atmosphere, which is now remarkable for its clearness.

The zodiacal light was first described about two centuries ago, and the various theories respecting it may be seen by referring to works on astronomy.

The subjoined description of this light, which we copy from the Encyclopædia Americana, will, we dare say, be acceptable to our readers, and especially to those who have alarmed themselves with the apprehension that this atmospheric phenomenon was a Comet, such as

> terror sheds
> On gazing nations, from his fiery train
> Of length enormous.

"ZODIACAL LIGHT—A triangular beam of light, rounded a little at the vertex, which is seen at certain seasons of the year, before the rising and after the setting of the sun. It resembles the faint light of the milky way, and has its base always turned towards the sun, and its axis inclined to the horizon. The length of this pyramidal light, reckoning from the sun as its base, is sometimes 40 degrees, and at others 150 degrees; and the vertical angle is sometimes 26 degrees, and sometimes 10 degrees. It is generally supposed to arise from an atmosphere surrounding the sun, and appears to have been first observed by Descartes and by Childrey in 1659; but it did not attract general attention till it was noticed by Dominique Cassini, (q. v.) who gave it its present name. If we suppose the sun to have an atmosphere, as there is every reason to believe from the luminous aurora which appears to surround his disc in total eclipses, it must be very much flattened at its poles, and swelled out at the equator, by the centrifugal force of his equatorial parts. When the sun, then, is below the horizon, a portion of this luminous atmosphere will appear like a pyramid of light above the horizon. The obliquity of the zodiacal light will evidently vary with the obliquity of the sun's equator to the horizon; and in the months of February and March, about the time of the vernal equinox, it will form a very great angle with the horizon, and ought, therefore, to be seen most distinctly at that season of the year. But when the sun is in the summer solstice, he is in the part of the ecliptic which is parallel to the equator, and consequently the zodiacal light is more oblique to the horizon. Laplace, however, has made some objections to this theory is his *Méchanique Celeste;* and Regnier is of opinion that it is owing merely to the refraction of the solar light by the earth's atmosphere.

The Madisonian adds:—

Cassini often mentions the great resemblance of the zodiacal lights to the tails of comets. M. Fatio has made the same observation; and Euler endeavored to prove them owing to similar causes. The extent of the zodiacal light, from the sun to its point, is seldom less than 45, and sometimes 150 degrees in length. The light seems to have no other motion than that of the sun itself. It brightness resembles that of the milky way; and from its shape we should suppose it to be the same meteor called *trabes* by the ancients, from its resemblance in form to a beam or rafter.

From the National Intelligencer.

THE STRANGE LIGHT.

HYDROGRAPHICAL OFFICE, March 10, 1843.

On Monday morning, the 6th, our attention was called to a paragraph in the newspapers stating that a COMET was visible near the sun at mid-day with the naked eye. The sky was clear; but, not

being able to discover any thing with the unassisted eye, recourse was had to a telescope, without any better success. About sunset in the evening the examination was renewed with great diligence, but to no purpose. As the stars began to peep out, observers were multiplied. Capella, Rigil, Beta (B) in the Bull, Delta in Orion, and Alpha (A) in the Hare, came to the meridian, and their transits were marked by one observer; while another, with close attention, watched through the fading light of the western horizon for the Comet and its tail. The last faint streak of day gilded the west, beautiful and delicate fleeces of *sonder* cloud curtained the bed of the sun, the upper sky was studded with stars, and all hopes of seeing the Comet that evening had vanished.

Soon after we had retired, the officer of the watch announced the appearance of the Comet in the west. The phenomenon was sublime and beautiful. The needle was greatly agitated; and a strongly marked pencil of light was streaming up from the path of the sun in an oblique direction to the southward and eastward; its edges were parallel. It was about 1 deg. 30 min. broad and 30 deg. long. Stars could be seen twinkling through it, and no doubt was at first entertained but that this was the tail of the Comet. The officer of the watch was directed to search the eastern sky with the telescope in the morning, from early dawn, and before, till sunrise. Nothing strange or uncommon was noted by him.

Tuesday was a beautiful day; the air was still and the sky clear. With the aid of the telescopes, a regular and systematic examination of the heavens was commenced. Every point of space within 15 deg. of the sun, in all directions, was passed and repassed over and over again, through the field of the telescopes, and minutely examined, in the hope of discovering the Comet itself. The entire day was occupied in thus exploring that portion of the heavens. In the afternoon a black spot, in shape not unlike a liberty cap, was discovered in the sun. But the day was fast declining, and with the fading light of evening the interest was too intense to turn away from the telescope for other and more exact observations upon this spot.

The sun was clear, gilding, as it sunk below the hills, a narrow streak of *wane* cloud, seen through the tree-tops beyond the Potomac.

The "tail" had appeared of great length for the first time the evening before; therefore we expected to find its length this evening greatly increased. It was a moment of intense interest when the first stars began to appear. The last rays of the sun still lingered on the horizon; and at this moment, just about the region of the upper crepusculum, a well-defined pencil of hairy light, was seen pointing towards the sun. At 5h. 41m. siderial time, the first measurement of length of the "tail" was taken; it measured 41 deg. to the horizon. At 6h. 19m. it had become most distinct. It was then 1 deg. 45m. broad, and 55 deg. long, not including the part below the horizon, which, supposing its terminus to be near the sun, could not, owing to the oblique angle which it made with the horizon, be less than 10 or 15 degrees more. It now commenced gradually to fade away, and in a short time had entirely disappeared. The morning observations were diligently renewed, but nothing could be seen worthy of note.

Wednesday, flying masses of "Cirrus stratus" occasionally obscured the sun; but preparations were made for determining the right ascension and declination of the "spot" on the disc of the sun; if, perchance, that spot should prove to be the nucleus of the comet. At noon the clouds broke away from the sun, and, though the glass was of feeble power, the well-defined dark spot was distinctly seen through it. The instrument itself was graduated to seconds, with micrometer readings; and, while the sun was in transit, observations for apparent right ascension and declination of the "spot" were made with the hope of reporting them the next day, and thence deducing its motions during the interval. A thickly clouded sky all of Thursday and so far of Friday (10 A. M.) with every prospect of continuance, has not permitted this hope to be realized. Most of the afternoon of Wednesday the sun was obscured, or but dimly seen through masses of intervening clouds. It, however, set in a clear and narrow streak of reddish sky, which was soon covered up by a bank of clouds 10 deg. high. Above this mass and upwards the sky was, for the most part, clear; and though evening was not favorable for the appearance of a "zodiacal light," the clear sky was watched with eager but confident expectation of again seeing the same beautiful phenomenon, which had excited our admiration and wonder for the two evenings previous. Accordingly at 5h. 54m. siderial time, and much to the delight of all present, this beautiful streak of light was dimly seen. In twenty minutes afterwards it was most distinct, spanning the southwestern sky with a faint nebulous arch, that reached away beyond Sirius to the East, and rested on Epsilon in the constellation of the Great Dog. It measured 85 degrees in length above the horizon, far below which it seems to extend. At 6h. 24m., just thirty minutes from its first appearance, this magnificent spectacle was lost in the gathering clouds.

The next morning observations in the east were again renewed as before, but without discovering any traces of nucleus, tail, or zodiacal light.

The sky has remained completely overcast up to the hour of writing, and no opportunity has occurred for repeating the observations, by which the motions (if any) of the spot over the sun's disc might be approximated.

Such, up to the present time, is the history, and such has been the appearance of this phenomenon, as observed here. The return of clear weather and the accounts of other observers are anxiously looked for. So far my own observations are not sufficient to enable me to hazard more than a mere conjecture, much less would they justify any positive assertion, as to whether this be the tail of a comet or not. If it be a comet, the probabilities are that it is receding from the sun, and will soon again be lost to us.

M. F. MAURY, Lieut. U. S. Navy.

From the Providence Journal.

THE COMET.

Mr. Editor: At a moment when the public curiosity is strongly excited by the appearance of an unlooked for and very remarkable Comet, a word upon the subject will not be out of place.

Six or seven days ago vague rumors from abroad by newspapers and otherwise, announced that a Comet was to be seen, and still more, that it was to be seen at mid-day near the Sun's disc. We very soon satisfied ourselves that the latter part of

this report was unfounded, and concluded, perhaps too hastily, the same of the former.

On the evening of Sunday, the 5th instant, the weather being very clear, the attention of many persons was attracted to a long, slender and slightly curved beam of light in the western sky, which appearing in a very transparent atmosphere and pretty well defined, though not remarkably bright, could not be confounded with those diverging beams which in a hazy air are often seen in the west after sun-set. On the evening of Monday, the 6th, as the twilight began to fade away from a perfectly clear sky, the same slender beam of light re-appeared with increased brightness. It was first observed by the writer at 7 o'clock. The beam commenced a little above the horizon, the ordinary exhalations not permitting it to be seen lower, at a point 10 or 15 degrees south of west, and with the aid of a common finder to a telescope could be traced 45 or 50 degrees of a great circle, making with the horizon toward the south an angle of about 35 degrees. The breadth at the lower point was about 2 degrees, or 4 times the diameter of the setting sun; it grew broader toward the upper end—not exceeding, however, about 3 degrees at the widest place. At the hour mentioned it was quite well defined, notwithstanding a strong twilight and the presence of the moon, seven days old. It was observed till a little after 8 o'clock, when the lower part had passed below the horizon, and the upper was lost in vapor and exhalations which always exist in the lower strata of the atmosphere.

There seems no reason to doubt that *this beam of light is the tail of a comet.* The body, or nucleus, if there be any, is probably too nearly in range of the sun to be seen. The direction of the tail would evidently pass near the sun's place, but whether north or south of it is not certain. We are authorized, then, to speak of this new visiter as a Comet; and it is certainly the most remarkable one which has been seen for at least a century.

We have said that it was unlooked for. Since the commencement of the Christian Era, five or six hundred different comets, or at least different *appearances* of comets, have been noticed. Of this number the orbits of about 140 have been computed. The orbits are nearly all of a parabolic form—that is, the path of the comet does not return into itself. When it has left our system, it leaves to return no more, unless its course should be disturbed by the attraction of some heavenly body and its orbit changed.

Three comets only are known to revolve in Ellipses, and to return at regular intervals. They are the following:

1. Halley's, which has a period of about 76 years. It passed its perihelion or nearest approach to the sun on the 16th of November, 1835. It will not again return till the year 1911.

2. Encke's, which has a period of 3 1-3 years, or more exactly 1207 days. This was last seen the spring of 1842. Its next return will be in the summer of 1845.

3. Biela's, which has a period of about 6 3-4 years, or more exactly, 2461 days. Its last return was in 1839. The next will be in 1846.

The first of these on its last appearance, though visible to the naked eye, was by no means conspicuous or impressive. Its appearance on some of its previous visits, is described as terrific and alarming in the highest degree. The two last named, Encke's and Biela's, are so exceedingly faint that it requires telescopes of considerable power to see them at all.

The present comet is remarkable for the length of its luminous train. The only conspicuous comet within the last century, or since the great comet of 1744, was that of 1811, the tail of which was 23 degrees in length. It has been observed that those comets which approach very near to the sun have their matter greatly diffused, thus producing long tails. On this principle, the present comet may be supposed to be near the sun.

The following is given by Arago as the lengths of the tails of some of the most remarkable comets which are upon record:—

Comet of 1811, length 23 deg.
" of 1689, length 68 deg. curved like a Turkish sabre.
Comet of 1680, length 90 deg.
" of 1769, length 97 deg.

Thus the comets of 1680 and 1769 might be in the horizon setting, whilst a portion of their tails would be in the zenith. Expressed in miles, the length of the former of these would be more than 96,000,000.

We, at first, entirely rejected the report that a comet was visible in the day time without the aid of glasses. Such testimonies have since been received as leave no reason to doubt that one or two days of last week the present comet was in such a position, and of such brightness, as to be seen at mid-day by the naked eye. This, though extremely rare, is not without precedent.

The Astronomer Lalande mentions one which was seen at the death of Demetrius, King of Syria, 146 years before Christ, which was *said* to be as large as the sun. Another appeared at the birth of Mithridates, which, according to the awe-struck chroniclers of that age, emitted more light than the sun and embraced the whole heavens.—But these accounts are evidently fabulous.

The following remarkable cases, in the opinion of the present eminent French Astronomer Royal, Arago, are well attested:

In the year 43 before Christ, a "*hairy star*" appeared which could be seen in *daylight with the naked eye.* It is hardly necessary to add that the superstitious Romans considered this as the deification of the soul of Cæsar, who had been assassinated a short time previously.

In the year 1402 after Christ, two comets of great brilliancy appeared. One of them was so bright that the nucleus, and even the tail, could be seen at mid-day in March; the other could be seen before sun set.

Cardan relates that in 1532, the curiosity of the inhabitants of Milan was greatly excited by "a star which could be seen at mid-day." Venus not being in a position to be seen at the time, the star of Cardan is believed to have been a comet.

"The beautiful comet of 1577 was *discovered,*" says Arago, "on the 13th day of November, by Tycho Brahe before sun set."

We have before alluded to the great comet of 1774. On the 1st of February this, according to the Astronomer Cheseaux, was more conspicuous than Sirius, the brightest of the stars; on the 8th it equaled Jupiter; a few days after, it was only equaled by Venus. On the 1st of March it was visible to the naked eye for several hours about mid-day.

The present comet, on supposition that the testimonies which have reached us should prove good,

will rank among the most extraordinary ever seen. It is no doubt moving with great rapidity. Should the evenings be cloudy for a week to come we may possibly not see it again.

Time was when the appearance of so strange a visitor, flaming through the firmament, spread universal terror and consternation. That time, we hope has, in a great measure, passed away to return no more. But the world is evidently not yet fully purged of the old leaven of ignorance and superstition. There would seem to be some among us who are incorrigibly bent upon making reason and common sense subordinate to the most childish fears, and the most whimsical and preposterous fancies. Predicted martyrdom at the tail of a comet would scarce surpass their credulity.—Whether there is any cure for such ignorance and fears we know not. It may be well, however, for such to consider that comets are a part of the creation which God made. They pursue their respective and appointed courses with as much order and harmony, in respect to the great laws of planetary motion, as do the members of our own little system. They are, so to say, messengers, from one system to another; and come back to us from the long travel of a thousand years, to announce to us that harmony and order pervade the universe.

Comets are not so rare as may at first be supposed. From 1802 to 1831, no less than 43 were seen. In 1826 five comets were seen, and all new ones. The probability is that about three new comets will be seen every two years.

A remark or two we intended to make upon the probability of the Earth's being some time or other destroyed by the collision of a comet. It is sufficient perhaps to say, that the *possibility* of such collision is admitted. The *probability* of it has been computed with great care. The result is given by Arago. It may possibly minister some consolation to those who dread such a catastrophe to be informed that the Earth minds her own business and gives to all idle wanderers an immensely wide berth. The chances of escape are 281,000,000 to 1 of collision.

The effect, moreover, of such collision *might* be very disastrous; but the real probability is that the Earth would bear off in the upper regions of her atmosphere the diffused matter of many comets without in the least disturbing the composure of us poor mortals in these nether parts.

We cannot but hope, therefore, that this interesting visiter will remain with us long enough to fairly make the acquaintance, and leave us to carry to other and distant portions of the universe the same welcome intelligence which it brings to us, that law, order, and harmony reign through boundless space. C.

Brown University, March, 9th, 1843.

COMETS—By Laplace.

Hon. Henry Meigs, a few evenings since, made some very interesting remarks at the exhibition of the Planetarium by the American Institute at Niblo's Garden upon the subject of "Comets." In the course of them he read the following passage, which he translated from the "Mecanique Celeste" of Laplace:

"We are certain," says Laplace, "from all the observations, and above all from the numerous comparisons of Maskelyne, which De Lambre made in order to construct the tables of the Sun, that the Comet of 1770 did not alter its sidereal year two seconds and eight thirds of time. Hence we are certain that its mass is not *one five thousandth part* of that of our Earth.

It results from the calculation of the preceding chapter, that this Comet *traversed the entire system of the Satellites of Jupiter, and yet it does not appear to have caused the slightest alteration among them!*

Comets do not *sensibly disturb* the motions of the Planets by their attractions.—But even if in the immensity of ages past, some of them have encountered those bodies, *as is very probable, it does not appear that the shock could have any great influence.*

If one of those Comets which may have encountered the Moon or a Satellite of Jupiter, had been of a mass equal to our Moon, there is no doubt it would have rendered their orbits very eccentric.

Besides, Astronomy presents us two other very remarkable phenomena which appear to date from the very origin of the Planetary system, and which an inconsiderable shock would have caused to disappear. I mean *the equality of the rotations of the Moon, and the librations of the three first Satellites of Jupiter.*

It is easily seen by the formula contained in the fifth chapter, as well as in the preceding chapter, that the shock of a Comet of one-thousandth of the mass of the Moon, must have caused very sensible alterations in the real librations of the Moon and of the Satellites.

We may rest assured, as to the true influence of Comets, Astronomers have no reason to fear any injury from them to the exactness of their Astronomical tables.

For The Tribune.

THE COMET.

Messrs. Editors: The "strange light" in the south-western sky, whether a Comet or other phenomenon, is certainly *not* the zodiacal light, to which most seem inclined to refer it.

The *zodiacal light*, according to *Biot*, "is always directed in the plane of the solar equator, which is nearly coincident with that of the ecliptic;" and "its name is derived from a zone called the zodiac, extending eight degrees on each side of the ecliptic, within which zone the lumineus appearance in question is *always comprehended.*"

The "strange light" under discussion, instead of being comprehended within the zodiac, on Saturday evening, touched with the northern side of the eastern extremity the star *Gamma Eridani*, which is fourteen degrees south of the equator, while the southern limit of the zodiac at the corresponding point is twelve degrees north of the equator, thus making the "strange light" twenty six degrees south of the limit within which the zodiacal light is "always comprehended." Those in the least acquainted with the heavens will appreciate the discrepancy when told that it pointed to Sirius, instead of to Aldebaran and the Pleiades.

The hypothesis of the zodiacal light being thus entirely out of the question, the decision lies between a Comet and some novel phenomenon, and the probabilities are almost infinitely in favor of the former. The only hesitancy is caused by the invisibility of the nucleus, but we may easily suppose this to be hidden by the mists which always dim the horizon, and we shall then be at full liberty to believe it a magnificent "Simon Pure" Comet. G.

THE "STRANGE LIGHTS" AGAIN.

HYDROGRAPHICAL OFFICE, March 11, 1843.

Advantage was taken of the clear sky this morn ing to examine the disc of the sun for the spot which so much attracted our attention and excited our speculation a few days ago. It was gone! Subsequent observations may enable us to determine whether or not this spot was the nebula of a Comet.

The wind was high and the sun unobscured after 10 A. M , and, though the day was very unfavorable for observations, search for the Comet was diligently renewed. It could not be seen.

From sunset till the stars came out, our telescopes were intently pointed to the western sky, with the hope of catching at least a glimpse of the Comet through the fading twilight. But we were compelled to content ourselves with admiring the wonderful beauty and grandeur of its train, the first traces of which were seen at 6h, 2m. sidereal time. At 6h. 28m. its greatest breadth was 1 deg. 40 min., as nearly as could be ascertained, for the bright moonlight rendered the measurement imperfect. The curve, before described, was again seen this evening, and in the same position, but much more indistinctly. At ten minutes after 9 o'clock the tail had not gone down, but had gradually faded away in the haze of the west.

Comparing my own observations with the accounts which have already reached us from different parts of the country, there appears now to be but little doubt but that this is the tail of a Comet, and that the head itself has been seen at mid-day in the East with the naked eye; if so, for length of tail and magnitude of nebula, it may be classed among the most remarkable that have ever appeared. In the year 43, B. C., historians tell us of a hairy star that was seen by day light. In the year A. D. 1402, two Comets were seen in broad day, and the tail of one of them at noon. Tycho Brahe discovered a Comet by daylight in 1577. The Comet of 1744 could be seen in the day time without the aid of glasses. Its tail, though not so long as this, was curved, and formed an arc of 90 degrees; though, for a part of the time, it had several tails, the length of which varied from 30 degrees to 40 degrees.

The Comet of 1680, the most celebrated of modern times, had a tail computed at from 70 to 90 deg. That of 1618 had the largest tail on record, 104 deg.; and this vies with that in length.

"The tail," says VINCE, in his Astronomy, "increases as the Comet approaches its perihelion; immediately after which, it is longest and most luminous, and then it is a little bent and convex towards those parts to which the Comet is moving; the tail then decreases."

If this Comet has obeyed this rule, it has already doubled the Sun, and is now on its way back, to be again lost in the regions of space.

M. F. MAURY. Lieut. U. S. Navy

[Nat. Intelligencer.

THE COMET OF 1843.—*Mr. Editor:* This day, Feb. 28, a comet of great brilliancy has been seen, visible through the day without the aid of a glass, and its brilliancy almost equal to that of Venus. Its situation is very near the eastern limb of the sun; its tail appears about 3° in length. It may be the comet announced some three months since in Europe; it was then traversing the constellation Draco; be it that or another it is of rare brilliancy. There are but three on record of sufficient brilliancy to be seen in the day season. The first was 43 years before Christ, and is called a "hairy star;" it was seen with the naked eye in the day time. The second was in the year 1402, and it was so brilliant that the light of the sun, at the end of March, did not hinder people seeing it at mid-day; both its nucleus and its tail were, to use the language of the day, "two fathoms long."—The third appeared in Feb. 18, 1744, and nearly equalled Venus in splendor, and many persons saw it at mid day without glasses. It may yet prove that the comet of to-day is the same as that of 1402. Three observations only are necessary to calculate its orbit, the elements of which, if previously registered, will enable astronomers to decide whether the comet under consideration has appeared or not.

Of 504 comets that have entered the solar system, 24 have passed between Mercury and the Sun, 47 within Venus, 58 between Venus and the Earth, 73 between the Earth and Mars, and 302 between Mars and the orbit of Jupiter, and no casualty has occurred to primary or satelite. The comet of 1770 passed through the system of Jupiter without producing the slightest effect; still many people are alarmed at the appearance of these erratic bodies, these rail cars of the stellar regions, the mystery of whose office and destiny makes their astronomy of intense interest.

With reference to the danger of a comet's striking the earth, we here add that the comet Encke, whose period is only 1,207 days, and nearest the earth of all the comets known, cannot come in collision short of a period of 219,000,000 of years, which calculation is based upon astronomical facts.

[New Bedford Mercury.

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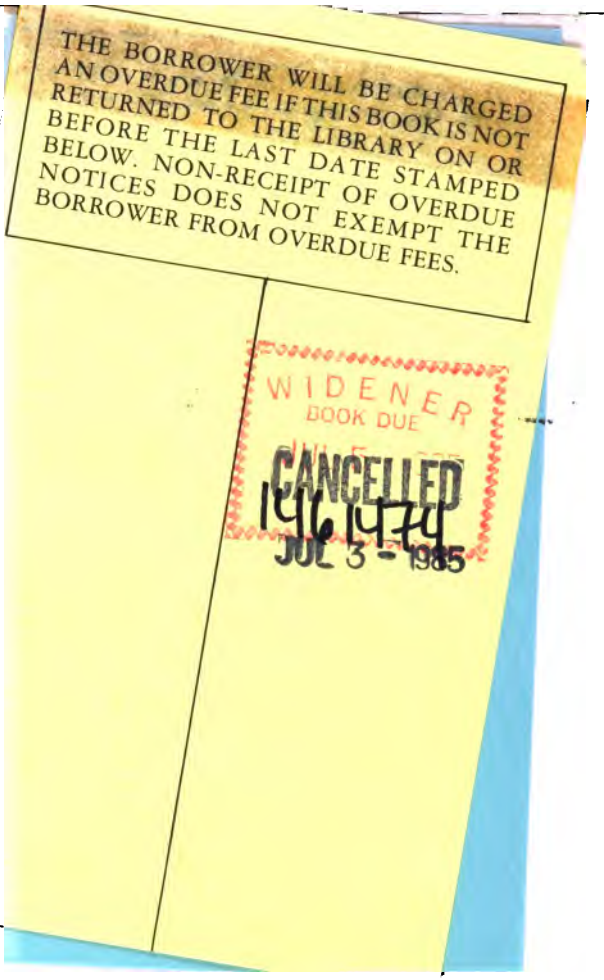
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